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The Conference on Historic Site Archaeology Papers 1969 - Volume 4

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The Conference on Historic Site

Archaeology Papers 1969

Volume 4



PART 1 Conference Papers

PART 2 Historical Archaeology Forum



**The Conference on Historic Site
Archaeology Papers 1969**

Volume 4

THE CONFERENCE ON HISTORIC SITE ARCHAEOLOGY PAPERS
1968

Volume 4, Part 1

Presented at the Tenth Annual Conference
Macon, Georgia

and

THE HISTORICAL ARCHAEOLOGY FORUM

Volume 4, Part 2

Stanley South, Editor

The Institute of Archeology and Anthropology
The University of South Carolina
Columbia, South Carolina 29208

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THE CHAIRMAN'S REPORT

The Tenth Annual Conference on Historic Site Archaeology was held in Macon, Georgia, at the Ambassador Motel on November 13, 1969. This Volume 4 of The Conference on Historic Site Archaeology Papers contains some of the papers presented at that conference, plus those submitted to THE HISTORICAL ARCHAEOLOGY FORUM. The pivot paper for the Forum for this year was Stanley South's paper "A Comment on the Relationship Between the State and Salvage Diving Operations", and a paper by Lee Spence which was presented at the 1968 Conference, but was held over for publication in this year's Forum. Lee's paper was entitled "Salvaging the Cargo of The Mary Bowers", and was a springboard for South's paper, which was in turn the focus for THE HISTORICAL ARCHAEOLOGY FORUM.

William Kelso's paper "Historical Archaeology in Georgia, 1968: Two Nineteenth Century Sites" was presented at the Knoxville Conference in 1968, but was inadvertently left out of Volume 3, so is included here. Iain Walker's paper "Note on the Bethabara, North Carolina, Tobacco Pipes" was not received in time to be presented at the Conference, and the same is true of T. M. Hamilton's paper "The Gunflints of Sept-Iles and Mingan" and its companion paper by Rene Levesque. Several papers presented at the Conference were never submitted for publication.

Thanks are due to those who participated in THE HISTORICAL ARCHAEOLOGY FORUM section of this volume, thus contributing to the continued success of this section. The maps for Stanley South's paper were supplied by the Institute of Archeology and Anthropology at the University of South Carolina, as was the typing of the manuscripts, as a cooperative gesture to the Conference. I would like to thank Carleen Regal and Mrs. Betty Williams for typing, Jane Rhett, Karen Lindsay and Alan Shoemaker for proofreading the copy, and Jim Frierson for assistance with drafting.

Stanley South, Chairman
The Conference on Historic Site
Archaeology

KAOLIN PIPE STEMS -- BORING IN ON A FALLACY

Lee H. Hanson, Jr.

Since 1954 when Harrington first published his observations on size changes in pipe stem bore diameters, this method has been one of the major dating techniques of historic sites in this country. In 1961, Binford proposed a straight-line regression formula based on Harrington's data which was designed to make it easier to date samples. Last year, while using this formula, I became aware of several shortcomings which affect the accuracy of the results. This paper outlines the problem areas and one possible solution to attain greater accuracy.

At the outset I want to make clear that I find no fault with Harrington's original observations about the trend of pipe stem bore diameters to become smaller through time, the method of measuring stem bores, or the percentage of frequency chart Harrington drew, which was based on his original data. My complaint is with what has been done to this data and the fallacies involved. These are: (1) The Binford assumption that pipe stem bore diameters follow a straight-line regression is erroneous; (2) No formula can give results more precise than the data on which it is based; and (3) I believe there were statistical and mathematical errors made in the computation of the Binford formula. The basic data used in this presentation is that provided by Harrington's chart, although the formulas to be discussed later have been tested on several published and manuscript sources.

Binford assumed a straight-line regression when he devised his formula, but Harrington's data does not support this assumption. In fact, it suggests a curved-line regression, with the regression fairly steep between 1620 and 1725 and gradually getting slower and slower after 1725 (Figure 1). In a straight-line regression the interval would remain constant per 1/64th of an inch change while Harrington's data shows that it drops from 32.09 years to 28.88, rises to 32.98, and then to 58.97. To partially compensate for these curves I have computed ten formulas for varying time periods from the Harrington data using the line of least squares method (Table 1). Within the limits of the data, each of these formulas is equally valid for the period it covers. Naturally, the shorter the time span, the more accurate the formula.

To use these formulas, pick the one valid for the time range which most closely brackets your sample, compute the arithmetic mean of your pipe stem bore diameters and substitute this figure for X. We will take up the standard deviation of the sample, which establishes the possible time range of the mean date, in a moment. The actual mean data of a sample could be anywhere within this range.

Since these formulas are still straight-line regressions their only claim to greater accuracy is that they deal with shorter spans of the curve and are less likely to deviate from the actual line as much as a single formula for the whole time span, A.D. 1620-1800. Of course, if you have an undocumented sample you must use the formula with the greatest latitude, Number 7, to establish the approximate date; something that could be done just as easily by comparing your raw data to Harrington's chart and estimating which periods it falls between. For example, a sample of 2070 stems from the Archer Cottage, Yorktown, Virginia, excavated by John Griffin (Griffin 1969), were suspected to date about the time of the American Revolution. Formula 7 gave a mean date of A.D. 1763.68 ± 16.74 years. Refining this date with Formula 10, within whose bounds it fell, we got a date of A.D. 1771.96 ± 20.93 .

These formulas, like Binford's, are straight-line regressions computed using the means of two time periods and the means of the bore diameters for those periods and calculating the amount of change until the bore diameter would reach zero, and the time interval involved in a decrease of 1/64th of an inch. The key work here is "means". When using a mean there should be a standard deviation in the formula based on the range of the bore diameters and size of the sample used in computing the means. My point again is: no formula can be more accurate than the data on which it is based. In this case, since means derived from Harrington's data are based on averages for time periods ranging from 30 to 50 years in length, the mean dates arrived at by formula should be in the middle of a time range of approximately 30 to 50 years, no matter what date is arrived at. The use of the standard deviation of the sample provides a statistical basis for computing the time range involved for the mean date. Because the periods we are dealing with here are so short, standard deviations will be of little value in refining dates except with undocumented samples. Two standard deviations are incorporated into these formulas because there is a 95% chance that the mean date will fit into the time period derived (Figure 2). Calculating to three standard deviations will only increase accuracy 4% more, while one standard deviation gives an accuracy of only 68%. The computation of the standard deviation is a basic statistical tool and not germane to our discussion here. It is sufficient to state that the standard deviation is doubled and multiplied by the average deviation of the samples used in computing the formula to give the probable maximum time range of the mean. By coincidence, both Fort Frederica, Georgia (Griffin 1969), and Silver Bluff, South Carolina (Eaton 1962), had samples with a mean bore diameter of 4.91/64ths. But the standard deviation gave the Fort Frederica sample a time range from A.D. 1716.42 to 1756.74 (actual dates, A.D. 1736 to 1750) while Silver Bluff dated from A.D. 1722.32 to 1750.64 because it was a more homogeneous sample and, hence, probably representing a shorter occupation time span. One further example is the Macon Plateau, Georgia (Irwin 1959), where the standard deviation produced a time span for the sample from A.D. 1681.47 to 1736.27, far longer than the postulated dates of occupation. Such a range is supported by the ceramic assemblage which suggests a long undocumented use of the area.

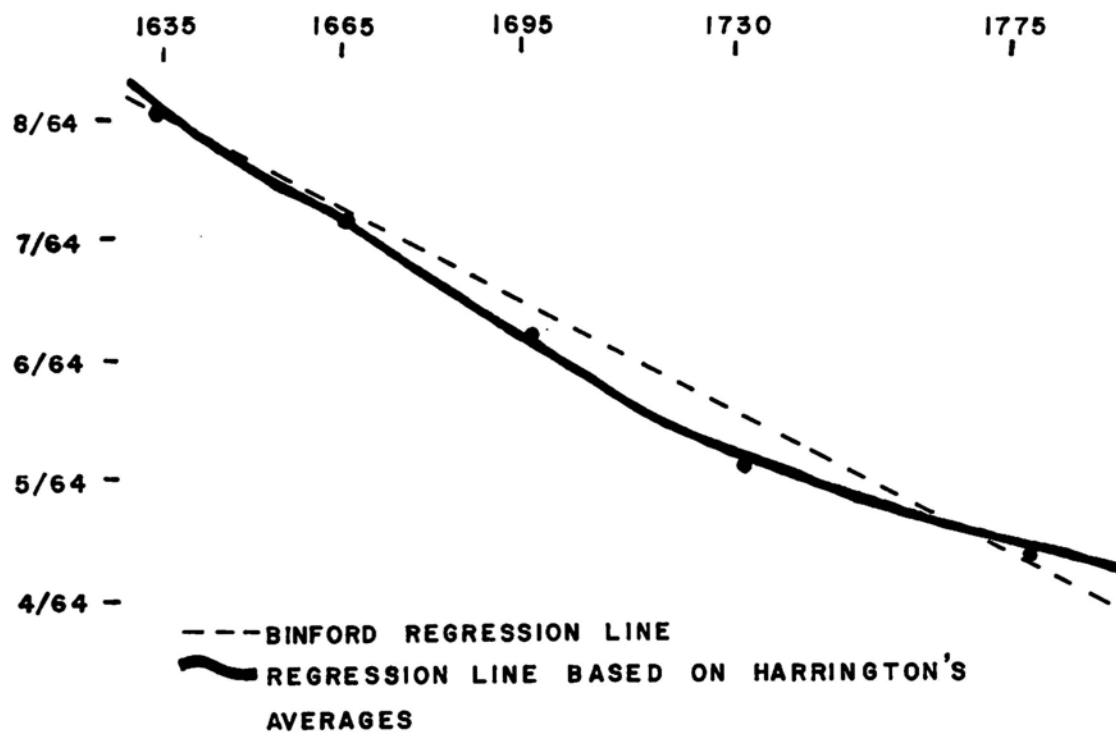
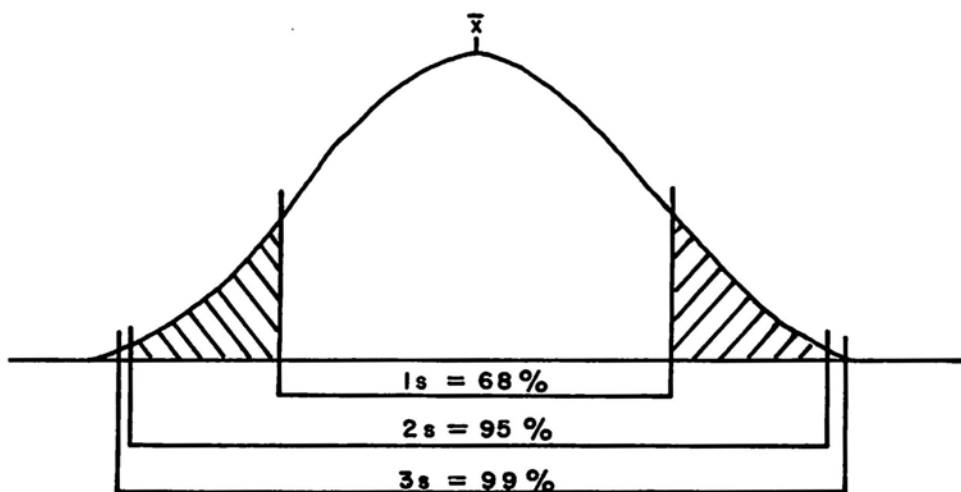


FIGURE 1



\bar{x} = MEAN OF THE SAMPLE

s = STANDARD DEVIATION

FIGURE 2

Table 1

REGRESSION FORMULAS FOR DATING ENGLISH KAOLIN PIPE STEM SAMPLES*

	<u>Time Range</u> <u>For Formula</u>	<u>Sample</u> <u>Date</u>	<u>Zero</u> <u>Year</u>	<u>Change</u> <u>Interval</u>	<u>Average</u> <u>Deviation</u>
1.	A.D. 1620-1680	Y =	1891.64	- 32.09X ±	(2s)(15.00)
2.	A.D. 1620-1710	Y =	1880.92	- 30.70X ±	(2s)(15.00)
3.	A.D. 1650-1710	Y =	1869.31	- 28.88X ±	(2s)(15.00)
4.	A.D. 1620-1750	Y =	1887.99	- 31.66X ±	(2s)(16.25)
5.	A.D. 1650-1750	Y =	1888.06	- 31.67X ±	(2s)(16.67)
6.	A.D. 1680-1750	Y =	1894.88	- 32.98X ±	(2s)(17.50)
7.	A.D. 1620-1800	Y =	1919.10	- 36.06X ±	(2s)(18.00)
8.	A.D. 1650-1800	Y =	1930.24	- 38.23X ±	(2s)(18.75)
9.	A.D. 1680-1800	Y =	1959.66	- 44.32X ±	(2s)(20.00)
10.	A.D. 1710-1800	Y =	2026.12	- 58.97X ±	(2s)(22.50)

*Based on "Dating Stem Fragments of Seventeenth and Eighteenth Century Clay Tobacco Pipes" by J. C. Harrington, Quarterly Bulletin of the Archeological Society of Virginia, Vol. 9, No. 1, 1954.

Users of these formulas should be familiar with the limitations of the original data before attempting to interpret the results of these formulas. Y is the mean date of the sample, X is the mean pipe stem bore diameter of the sample in 64ths of an inch, and s is the standard deviation of the sample. 2s = 95% reliability.

A major problem is that the bore diameters changed at such a slow rate that our samples do not adequately reflect that change because they generally represent several years' accumulation of specimens. Perhaps a good statistician will come along some day and show us how to cut down this time range for the mean by proving the reliability of the formulas. In the meantime, we need more closely dated samples to use in recomputing these formulas. The more samples the formulas are based on, the more accurate they will be. At the present time none of these formulas are based on more than five samples.

The Binford formula should have appeared as one of the ten formulas on Table 1. It was quite a shock to discover in a routine check of Binford's formula that it could not have been derived from Harrington's data by the method Binford outlines. He states, "I computed from Harrington's percentages a straight-line regression and arrived at a formula which would allow me to substitute values from any archaeological sample into the formula and determine an absolute date which would be the period for sample accumulation. This I was able to do by using Harrington's original percentages and converting them to mean hole diameters for the given time period." It amazes me that no one has checked the accuracy of this formula before, especially since it seldom gives the mean sample date it is supposed to. Most "tests" of the Binford formula have ended up assuming that the formula is correct and any discrepancies must be due to other factors such as population increase, sampling error, or inadequate documentation of the occupancy of the site. Now we must introduce another factor -- the Binford formula is statistically inadequate and mathematically incorrect! I spent days trying to work out the Binford formula from the Harrington data on which it was supposedly based both by the line of least squares method and as a simple regression formula where a straight line is known. Since I could not calculate the Binford formula from the Harrington data, what follows is pure conjecture on my part as to how that formula was derived. To succeed, one must ignore all but the earliest and latest of Harrington's five time periods and make three mathematical errors. First, one has to use .026 as the change factor per year, which is attained by rounding off the mean bore diameters of the earliest and latest of Harrington's time periods to one decimal place instead of two. The actual change factor without rounding off should be .02664. Using .026 will give a change interval from one diameter to the next of 38.46 years which must be altered to read 38.26. Finally, the number of years to a theoretical zero, again using .026, is 163.85 which must be added to 1768 instead of the proper date of 1775 to arrive at the zero point of 1931.85 that Binford attained. Either he added the figure 156.85 to the median date or did not use the correct median date for computation.

Having suggested how these errors account for the Binford formula, we are forced to admit that without them, the formula would never have come as close as it did to giving accurate results. I am well aware that the "spectacular" results attained with this formula may keep it around for some time despite the points I have raised. However, I believe these new formulas are statistically more valid and will prove more accurate, when properly used, to Binford's calculation and it is probably well that these questions be raised now when other formulas are available.

I have stated before that we need better documented samples to refine the formulas further, especially in the 18th century where the present ones are practically worthless because of the time ranges involved in Harrington's data. Eventually, all of Harrington's data should be replaced by documented samples. There are four criteria that must be met by these samples:

- (1) They must consist solely of English pipe stems. Preliminary evidence suggests that Dutch pipe stem bore diameters have a different regression line and mixture would alter the formula.
- (2) Samples must be from contexts with known beginning and end dates. That is, there must be accurate and irrefutable documentation on the sample.
- (3) The sample should be statistically large, over 30 stems. This assumes that the samples are random and approximately a normal curve. Although I would need the original data to prove it, I strongly suspect that the various means Audrey Noël Hume derived from her data are not statistically significant and could have varied as much as they did by pure chance. If a sample approximates a normal curve, increasing its size will not significantly increase its accuracy. On the other hand, however, the larger the sample the more likely it is to approximate a normal curve.
- (4) Finally, there must be no chance of contamination by material from other time periods, either through intrusions or sloppy excavation.

New samples will allow us to compute formulas for shorter time spans, thereby increasing accuracy by further negating the effect of the curved regression line. A number of documented samples may enable us in time to abandon this method entirely in favor of some other method better-suited to the nature of the material. Such alternatives do exist but require numerous documented samples.

There are four problems relating to dating pipe stem samples that should be discussed in detail. These are: factors affecting the rate of change of pipe stem bore diameters, the time span of sample accumulation, the context of the sample, and how these problems compound one another through the use of a formula.

As Harrington pointed out in his initial paper on pipe stem changes (Harrington 1954) and reiterated in his comment on Chalkley's "critique" (Harrington 1955), the length of pipe stems generally increased throughout the Seventeenth Century, reached a maximum in the early Eighteenth Century, and were made shorter toward the close of the Eighteenth Century, all in accordance with the demands of the times, or "fashion" if you will. This probably had an effect on the size of the stem hole, longer stems requiring smaller wires for reaming out the hole and, hence, producing

smaller holes and thicker stem walls. Another factor may be an improved technology which could produce wires with smaller diameters. Unfortunately, we do not control the information necessary to measure the effect of this and the evidence from the close of the Eighteenth Century suggests that it was no longer a factor. A third factor is the difficulty one would experience in smoking a pipe with a bore diameter of less than $3/64$ ths of an inch, regardless of stem length. This would definitely affect the minimum bore diameter permissible and might be another explanation of why the bore diameters began increasing in the late Eighteenth Century. A fourth factor over which we have no control is improved strains of smoking tobacco and its preparation which may have permitted these changes, or indeed, necessitated them. Finally, we must consider the increase in bowl size and the amount of tobacco it could hold.

Any or all of these factors, need for thicker walls because of stem length, improved wire-making technology, need to improve the "draw", improved tobacco, and larger bowls did not produce overnight or constant changes in bore diameters. Both the pipe makers and the buyers were subject to the process of culture change. That is, while most makers and buyers were following current fashion, or the cultural norm as the anthropologist would put it, there were a few, the innovators, making and using pipe styles which would become the fashion of the future and a "conservative" faction still using pipes which had ceased to be fashionable. In addition, it is unlikely that styles which were popular in some areas were popular in all or that change was taking place in all areas at the same rate. The English, in particular, catered to colonial markets with regional tastes. I am here making a rather broad assumption that any change in pipe style could be reflected in pipe stem bore diameter because of the technological changes necessary to produce new styles. I believe that such an assumption is warranted based on our present accumulation of data. In any event, we are faced with a broad spectrum of intangibles which may be reflected in the diameter of pipe stem bores. We may ignore them during the analysis phase of our studies but we cannot ignore them during the interpretive phase.

This brings us to the next major problem, the time span of a sample without complete documentation. Because we do not know the fashion or technological trends of any area for any period (except bore diameters get smaller through time until the late Eighteenth Century when they again get larger), we can only make educated guesses about the time span of any one sample. For example, the Jamestown ice pit, Structure 128, contained six distinctive fill zones and Cotter concluded that it had been filled deliberately shortly after A.D. 1676 with refuse and the remains of a nearby structure destroyed in that year (Cotter 1958:147-150). For my analysis of the pipe stems I grouped the six zones into lots of two and computed the mean bore diameters for the three resulting samples. There was an internal consistency to the samples with a change in the mean from 8.17 to 8.12 to 8.09/64ths of an inch. At first glance, this evidence would suggest that it took about 2.5 years to fill the pit. However, I subjected the data to a Null hypothesis test which showed that the change, even though consistent, could have been due to chance and, therefore, the pipe stems do not alter the original hypothesis that the pit was filled within a short time. The

point is that even though a deposit is probably laid down within a short span of time it may show quite a variety of bore sizes. The ice pit had a range from 6/64ths to 9/64ths of an inch. Some of this is probably due to the secondary nature of this deposit, but samples ranging over only two diameters are sufficiently rare to suggest that any population (which might be a single individual) would have in its possession, and subsequently discard, stem fragments of more than one size or bore diameter. For this reason, and the absence of any accurate means of assigning an absolute calendrical date to the mean bore diameter of a sample, we cannot measure absolutely the length of time to took a sample to accumulate. At best, we can assign dates which bracket the time range during which the sample probably accumulated. The actual alpha and omega dates of the sample should fall within this range but with the present state of our knowledge formula users will quickly discover that such dates can be used only to corroborate other dating evidence and not as a means of dating artifacts, features, or strata. For this reason, care must be taken to identify the context of the sample in the site report so that readers will be aware of the significance of the date, or its lack thereof. Furthermore, samples should be defined by their context and not lumped together into a single polyglot sample, because such a conglomeration cannot be used to date anything, not even the total time span of the site. This is because all time periods of the site might not be equally represented and some might not be represented at all. A sample collected from the surface of a site can suggest the probable occupation period of that site but cannot be used to give alpha and omega dates for the occupation because all periods of occupancy might not be represented by pipe stems (or at least pipe stems on the surface), the occupancy may have been intermittent rather than continuous, and the formulas for arriving at such dates are not accurate enough yet.

At Tutter's Neck, where a more complex situation existed, four samples were tested (Noël Hume 1966:52-54). Pit B, which was intruded by the kitchen foundation, contained a relatively early sample of pipe stems. Pits C and D were within the kitchen and also preceded it, being capped with layers of burnt clay. These contained samples significantly later (statistically) than Pit B, but not significantly different from each other. The final sample was taken from a six-inch stratum overlying a burnt clay layer outside the kitchen which appeared to have been disturbed by bulldozing. This sample dated earlier than Pits C and D but later than Pit B. On this basis we can say that the stratum contains a redeposition of earlier material or the burnt clay underlying it may be similar but is not identical to the burnt clay layers sealing Pits C and D. Mr. Noël Hume properly resorted to evidence other than pipe stems (that is: evidence of bulldozing) to make his interpretation of a secondary deposit but sometimes such evidence is lacking, overlooked, or ignored. This is one situation in which pipe stem samples can be useful by pointing out the anomalies. If Stratum A overlies Stratum B but its pipe stems date significantly earlier, it is time to start looking for an explanation and "family heir-looms" is not a very substantial one.

We have referred to statistics several times in this paper but have tried to avoid getting sidetracked until other matters were disposed of,

There are four basic statistical techniques that can be applied to the data derived from pipe stems. These are: computation of a mean, computation of a standard deviation, use of the line of least squares method of computing a regression formula, and use of Null hypothesis tests to test for statistically significant differences between means. The formulas for these statistical techniques and their mathematical bases can be found in standard statistical textbooks and I want to dwell here only on the uses and limitations of the techniques.

Basic to all applications of statistics to pipe stems are two assumptions, that the sample is representative of the total population for the given context, and that the sample has a normal distribution which, when plotted graphically, will show a bell-shaped curve for the bore diameters represented (Figure 2). The first assumption has to be accepted as an article of faith but the second can be checked graphically and there are statistical techniques to correct for samples where the apex of the curve is at one extreme of the range.

The mean of a sample is easily calculated and its limitations are seldom considered. For a mean to be accurate the sample must meet both assumptions regarding representation and normal distribution. There are statistical techniques to compensate for small samples and the size of the sample is irrelevant provided it meets these criteria. Extremely important is the fact that we are dealing with two means in the formulas, the mean bore diameter and the mean date of the sample. The standard formulas are computed from the correlation of the two and once the formulas are set up either may be used to determine the other. The mean bore diameter of a sample is an irrefutable fact; it can be measured, calculated, and recorded. The mean date for a sample is intangible and must be inferred from documentary sources and archeological context. To know from documents that a site was occupied from 1732 to 1745 does not help to date a sample from that site unless that sample can be tied to documented features and the possibility of undocumented occupation can be ruled out. To use a mean sample date derived from one of the present formulas as evidence for the occupation of a site at a particular time is unwarranted unless the reader is made aware of the limitations of the data.

We can overcome this problem to some extent by using the standard deviation of the sample. This is applicable only to the bore diameters and is calculated from their range within the sample. The principle on which this is based is that one standard deviation from the mean will encompass 68% of the sample on a normal curve (Figure 2). Two standard deviations will encompass 95% and three will encompass 99%. I consider two standard deviations to be sufficiently accurate for our purposes because of the slow rate of change, the presence of two or more bore diameters in use at any given time, and the average deviation of the formulas which is necessarily large because of the time spans of the samples on which they are based. When we can demonstrate the accuracy of the formulas, or change them to make them more accurate, we may be able to justify changing the number of standard deviations. To convert two standard deviations of the mean bore diameter into a time span for

the sample we must multiply them by one-half the average time span of the samples used in computing the formula. This gives a plus or minus factor to be tacked on to the mean sample date as the probable time range of the sample and the actual mean date has a 95% chance of lying within this range. On the few documented samples where we have used this method thus far, the actual time range has correlated very well with the average deviation, justifying our use of a two standard deviation range. The average range is derived from the documented ranges of the samples used in computing a formula. It is used as a factor in converting the standard deviation of the bore diameter of a sample to a date range. Such a method is sound mathematically, but should be used with caution. The range thus obtained is only an indication of the time span of the sample. It does not date the sample, much less the context in which it was found. For example: at Silver Bluff, South Carolina (Eaton 1962), the documented occupation of the site was A.D. 1716 (?) to A.D. 1780 with a mean date of A.D. 1748. The pipe stem sample dated between A.D. 1722.32 and A.D. 1750.84 with a mean date of A.D. 1726.58. Unfortunately, we do not know the provenience of the sample so that it cannot really be related to the documented dates at all. Whether the sample came from a feature or the surface of the site would make a big difference in our interpretation of the sample date. The important thing is that the sample time range fits within the documented time span of the site, even though the mean dates are eleven years apart.

The use of a formula compounds the problem of arriving at an accurate date, but has one advantage in that we are dealing with a set of standards and do not have to resort to arbitrary methods such as fitting frequencies to bar graphs. Furthermore, the formulas have the potential for refinement with new data. This alone makes it worth our while to continue using the formulas despite their present limitations. In summary, I would like to review these limitations. First and foremost, we must assume that our samples are representative of the population for the context it is taken from. If it is representative, the size of the sample is irrelevant and if it is not, we have no way of proving it; although there are signs, such as an abnormal curve, which would lead us to doubt the validity of the sample. Second, in using standard formulas derived from several samples we must demonstrate that the samples have a normal curve in order to use the means in calculating the formulas and the standard deviations in calculating the time ranges of the samples. This can easily be done by visual inspection. It must be borne in mind that this second point is based on the premise that all cultural phenomena, be they ideas or objects, are constantly changing (but not necessarily at a constant rate) and that they each have an increase, a peak, and a decline in popularity through time. Each sample represents a portion of a population which is itself only a segment along a time-line representing the existence of the idea or object class. Third, we must understand that we are dating the sample and must infer the date of the sampler's context from the sample or from other sources. This distinction between the sample and the context in which it is found must be made clear and such factors as mixed or redeposited samples must be taken into account even before the sample is dated and this date is

compared with other evidence. Furthermore, it should be made clear just what context the date applies to; that is, whether it came from a well-defined pit, the surface of a disturbed site, or represents an accumulated sample from several distinct features and strata in a site. Only in light of this information can a sample date be interpreted. Finally, it must be kept in mind, and so noted in the report, that the formulas will give us only the approximate mean date for a sample and the approximate time range during which the sample could have accumulated, based on the data from which the formulas were constructed. As we refine the formulas these dates are likely to change. In no case should dates obtained by this method be referred to as absolute dates without any margin for error; that is, the time range of the sample.

This paper was not written to bury the Binford formula and dance on its grave. Its purpose has been to point out the problem of dealing with a curved-line regression using a straight-line regression formula, and to present my solution to this problem, a series of formulas dividing the curve into segments to negate the effect of the curve. We desperately need better-documented samples to replace Harrington's percentages so that we can study the regression curve in greater detail. I welcome any ideas and suggestions you may have for improvement.

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HISTORICAL ARCHAEOLOGY IN GEORGIA, 1968:
TWO NINETEENTH CENTURY SITES

William M. Kelso

The Georgia Historical Commission's Department of Archaeology conducted excavations at two of its fourteen historical sites in 1968; the first at Fort Jackson, Savannah, and the last at Traveler's Rest, Toccoa. Both were scenes of heavy occupation throughout the nineteenth century. This paper, however, will not be an attempt to present a detailed account of the historical background, the excavation procedures, nor the structural evidence found at these two sites. Instead, this presentation will concentrate on illustrating the tightly dated artifact collections found at both sites in order to show the types of artifacts used at two given places and times during the nineteenth century, and to suggest what characteristics of certain artifact types an archaeologist might look for when excavating or studying similar sites. It is hoped that through this and similar presentations by others, we can continue to build a dated artifact reference for the complex nineteenth century with at least as much validity as we have already done for the eighteenth century. Therefore, the first portion of this presentation will briefly dwell on enough of each site's history and excavation to establish the credibility of the dates assigned the artifacts while the later section will be essentially an illustrative and comparative presentation of the objects recovered.

Traveler's Rest

Traveler's Rest is located six miles east of Toccoa, in northeast Georgia. Historical records indicate that in 1785 the land had originally been granted to Major Jesse Walton, founder of Jonesboro, Tennessee, and that Walton was killed by Cherokee Indians on his land in 1789. The area eventually went to Devereaux Jarrett in the 1830's, and the house became a wayside inn for those traveling the King's Highway. Documents do not say anything about the structures at the site; hence, architectural study and archaeology are playing a vital role in its interpretation.

Excavations were carried out at the site in the summer of 1968, hoping to not only learn something about the nineteenth century inhabitants but also to date the various periods of construction of the main building and define the location and functions of the various dependencies.

The subsequent excavation work uncovered the field stone foundations of the original smokehouse and of its two floor levels, the earliest suggesting occupation no earlier than 1830. A small two story outbuilding referred to locally as the loomhouse was also investigated, likewise revealing multiple dirt floor levels, the earliest of which contained a

with plans to establish a maritime museum in the old casemates. To make the interior parade ground more suitable for visitation, however, a serious drainage problem had to be overcome. Occasionally, the water table inside the fort stood six inches above ground, and reasoning that the troops could hardly have held reviews in ankle-deep water, an archaeological search for the old drains was carried out.

Two areas became immediately eligible for excavation in that each could logically contain the sought after drain, a central sunken area to which the water already seemed to drain, and two square bricked vaults at the rear of the parade ground. The central hole turned out to be the well, complete with brick terrace and sandstone cap. Using five foot corrugated pipe sections as shoring and a diaphragm pump, the well was excavated revealing a wooden and well liner and very few artifacts. Apparently, the shaft had been kept clean until very recent times, due to the fact that it had been completely enclosed and capped by a fountain as revealed by a Harper's Weekly drawing made in 1865. At some later time the fountain had been removed and the south side of the well had collapsed. After the excavation, it was decided that the empty cavity could serve as a catch basin and since then a cap and a sump pump have been installed, successfully lowering the water table and drying out the parade ground.

The brick vaults at the rear of the fort were also excavated and once again a pump was needed full time during the work. Drawings made in 1842, show that these shafts were to serve the fort as the "privy vaults", a rather unhappy prospect for the excavators. But the lack of prestige was soon forgotten as it became progressively clear that after the building's superstructure was gone, the shafts became a dumping area for domestic trash. Everything discarded at the vault remained constantly wet, therefore, even the discarded organic items were preserved. Because we were dealing essentially with "mud", all spoil removed from the vaults was hosed through a screen. The vaults turned out to be eight feet deep and included small drainage tunnels at the bottom leading into the moat.

At a depth of five feet into the vault fill, a layer of burned boards and timbers was found (See Fig. 2), probably the remains of the fire which destroyed the fort's buildings just before the Union capture in late 1864. The latest datable artifact recovered above the burned level and below the surface disturbance was a bottle dated 1878. Therefore, the group of artifacts found in these vaults was probably discarded after the fire (ca. 1865) and as late as or after the bottle date, 1878. The fort was occupied during this period by Union forces and civilian and military laborers working on the construction program of the 1870's. Subsequently, the fort was temporarily abandoned about 1878.

Therefore, the efforts of historical archaeology in Georgia, 1968, produced two stratified and datable artifact groups deposited during short periods of time during the nineteenth century, about fifty years apart. Examined together, the collections show interesting parallels in artifact types, but, more importantly, hint at what effect those fifty years had on similar objects.

small storage pit and the remains of a wooden step. The artifacts and the architecture suggested that this building probably was used as the dairy in the 1820's.

An architectural study of the main house is revealing that this structure had undergone at least two major periods of construction evidenced by differing materials, construction methods and carpenter's marks. It was hoped that archaeology could date these periods. Therefore, the major emphasis of the excavation was concerned with the investigation of the main house.

The obvious place to begin was at the builder's trenches, but a cross section through one of the foundation walls quickly showed that very late underpinning and erosion had swept almost all of the trenches away. The builder's trenches being eliminated, an attempt was then made to date the construction of the various chimney's by locating and excavating the scaffold holes. Twelve holes were located but no datable artifacts were found. However, the fact that scaffold holes were located on the north side of the existing central chimney (now under the north section of the main house) confirmed the architect's conclusions that the south section of the main house had been built first.

The entire dust surface under the main house was then scraped down in the hopes of locating drip lines, postholes, or any indication of previous occupation and this operation proved to be more successful. A worn path was found leading from the basement kitchen toward the south portion of the main house and a backfilled root cellar appeared under the south end of the earliest section of the house. The feature was seven and one half square and two and one half feet deep surrounded by miscellaneous gouges apparently dug under the house by animals. A shallow slot had also been dug leading from under the front porch to the edge of the cellar the purpose of which remains a question mark. Both the slot and the pit had been backfilled level with the remaining under-surface of the house. Immediately below a shallow level of modern dust, a layer of burned rocks and chunks of dried red clay (clay chinking) had been thrown into both the cellar hole and the slot, (See Fig. 1) probably debris left over from the dismantling of a chimney. Below the chimney debris, the cellar also contained a level of dark humus flecked with wood ash. Both lower levels contained early nineteenth century domestic refuse and significantly, both layers ran under the rock hearth foundation of the existing main house. The most datable artifact recovered from the cellar fill, an American Corps of Artillery button made no earlier than 1815, (See Fig. 12, lower left) dated the deposit. A fine collection of early nineteenth century ceramic, metal and glass objects was recovered from this feature.

The other Commission site excavated in 1968, Fort Jackson, lies two miles east of Savannah on the Savannah River having protected the eastward water approach to the city since the early nineteenth century. In 1808, construction of the present masonry structure for the United States Government was begun with various expansion programs in the 1840's, 1860's and 1870's. The Commission acquired the site in 1965, and has proceeded

The Artifacts

First, the ceramics. The common pottery type found at Traveler's Rest consisted of the usual featheredged pearlware in green or blue, very delicate and unmarked (See Fig. 3), or creamware in either the royal pattern or a relief edge design. At Fort Jackson the ceramics consisted almost entirely of the familiar ironstone china (See Fig. 5), more properly called graniteware, the type exported to the United States by the ton in a variety of forms during the third quarter of the nineteenth century. Many of the pieces bore maker's marks and patent lozenges designating pottery type, the manufacturer, place of manufacture, and month, day, and year of patent. One such plate was made by the Davenport pottery patented on October 5, 1863 (See Fig. 5, center). Two pieces, a saucer (See Fig. 5, middle right) and a mug (See Fig. 5, lower right), bore marks (See Fig. 7, left and center) of the American pottery of Edwin Bennett of Baltimore, in business from 1856 to 1890.

Cobalt blue handpainted pearlware, "Gaudy Dutch", was also found at Traveler's Rest along with polychrome handpainted pearlware banded in brown (See Fig. 4), whereas, at Jackson, the more decorative pieces were embossed with relief designs and colored glazes such as teapots bearing the familiar "Rebekah at the well" scenes so popular during the second half of the nineteenth century or "yellow-ware" bowls sponged and banded in blue.

Stoneware was recovered at both sites, fragments of English brown stoneware, and a mottled greenish-brown type at Traveler's Rest, and a similar greenish-brown type at Fort Jackson. One such jug (See Fig. 6) found at Fort Jackson bore the mark (See Fig. 7, right) of a factory located in Trenton, South Carolina, a small town near Aiken.

Some clay tobacco pipes of the Traveler's Rest period were the reed stem type made of local clay in a plain form or a molded human head form (See Fig. 8, no. 2 and no. 3). Both have some similarity to those made by Gottfried Aust and Rudolph Christ in the second half of the eighteenth century at Bethabara, North Carolina, some of which were traded to the Cherokees. One pipe was made of soapstone (See Fig. 8, no. 1), apparently, in an attempt to copy an early eighteenth century English form. Surprisingly enough, not the first English white clay pipe fragment was recovered in all the digging at Traveler's Rest. Apparently, the practice of molding reed stem pipe bowls into human head forms continued into the 1870's, for one such pipe was found at Fort Jackson along with a plain, cross-hatched example. Several forms of white kaolin pipes were used at Fort Jackson exhibiting a variety of designs (See Fig. 9). The practice of relief ornamentation was carried to extremes in some cases (See Fig. 9, no. 2), whereas, one beautiful example of mold work (See Fig. 9, no. 1) depicted a steam engine and a side wheeler steam boat on opposite sides of the bowl; perhaps, a piece commemorating fifty years of steam power. Several ribbed and heeled pipes made by the McDougall firm of Glasgow (See Fig. 9, no. 3) were also recovered along with the usual initialed T. D. pipes (See Fig. 9, no. 4, no. 5, no. 6), the letters either stamped or molded in relief.

A variety of metal items and small finds were recovered from both sites, and apart from nails, buttons were the most numerous. The Traveler's Rest buttons were mostly made of brass with only a few pewter and bone types present. Several were plated, whereas, the sixty buttons of the post Civil War period found at Jackson were predominately made of porcelain. A typical button of the Traveler's Rest period (See Fig. 12, right) was 13/16 inch in diameter, stamped flat or slightly convex on the front side, had some type of relief lettering on the back, and featured soldered loop fasteners. One example of this type was made in London (See Fig. 12, upper left). On the other hand, the porcelain buttons at Fort Jackson were mainly 7/16 inches in diameter, had four countersunk fastening holes, and were convex on both sides. An example of a military button of each period was found at both sites: a corps of artillery button of the 1815-1821 period (See Fig. 12, lower left) which dated the artifact deposition at Traveler's Rest, and a plain shield eagle button at Fort Jackson. a type used by the army from 1855 to 1872.

A few nails were recovered at Traveler's Rest, most rusted beyond recognition. However, two relatively preserved examples serve to illustrate the transition of manufacturing techniques that occurred during the first half of the nineteenth century: a machine cut nail with a hand forged head and a machine cut and headed nail. Thousands of nails were found at Fort Jackson, both brass and iron, and many in an excellent state of preservation. Only representative samples have been analyzed, but so far they are all common cut, machine made nails of various sizes. No wire nails were recovered.

A comparison of the common straight pins from each site also reflects technological advancement. The pin found at Traveler's Rest had a hand made knotted head so typical of the eighteenth century while the Jackson pins had been completely machine made.

The padlocks of the early nineteenth century found at Traveler's Rest were either heart shaped (See Fig. 10, no. 3 and no. 5) or rounded in shape (See Fig. 10, no. 4), somewhat reminiscent of seventeenth century ball padlocks while the one lock of the post Civil War era was heart shaped (See Fig. 11, right) with a brass keyhole guard and liner.

Iron pintles were found on both sites, the earlier examples having been hand wrought and made to be driven into the door frames (See Fig. 10, no. 1 and no. 2), whereas, the late forms from Fort Jackson (See Fig. 11, left) were machine made in two parts and made to be fastened with screws.

Samples of cutlery were found at both sites, although fragmentary in the case of Traveler's Rest. Bone handles were utilized in both periods, and the practice of silver and tin plating was used extensively on the Jackson spoons.

Little was found to illustrate the types of firearms used at Traveler's Rest in the 1820's with the exception of one pistol ball. However, the various types of cartridge cases recovered at Fort Jackson testify to the variety of weapons available by the post Civil War period.

Cases from a Henry repeating rifle made by Winchester, a 50-70 center fire rifle, a 50 caliber rolling block Remington Navy pistol, a Colt 45 pistol, and an early rim fire 22 made by the Phoenix Cartridge Company represent some of the types found at Jackson.

The glass items from each site, although very fragmentary, once again at Traveler's Rest, included enough of a type variety to furnish considerable insights into at least late nineteenth century glassware. Fragmented free-blown, dark glass wine bottles were found at Traveler's Rest (See Fig. 13) featuring high conical kicks, a convex neck, and double ridged string rim so typical of the late eighteenth-early nineteenth centuries. At Fort Jackson, on the other hand, seventy-four unbroken bottles were recovered from the fill, displaying every shape and color imaginable. However, the large liquor bottles exhibit some similar traditional characteristics to their early nineteenth century counterparts (See Fig. 14, no. 1, no. 2, and no. 4); i.e., cylindrical body, tapering neck, double ridged string rim and raised basal kick. One bottle was even embossed with a seal (See Fig. 14, no. 1), that of E. P. Middleton, Philadelphia, 1825. However, unlike the earlier free-blown bottles found at Traveler's Rest, the Jackson liquor bottles were blown in either two or three piece molds as far up the body as the area just below the lip or string rim. In one instance, the manufacturer's name and location were molded around the base (See Fig. 14, no. 2); i.e., the Dyottville Glass Works, Philadelphia, a firm in business periodically from 1833 to 1926.

The Jackson beer and ale bottles (See Fig. 15) display, basically, the same style characteristics with the exception of color, and in some cases lip design, although black or dark olive green glass was still used for some ale bottles (See Fig. 15, no. 1). Soda and mineral water was popular during this period as several examples of the bulbous necked "pop" bottles in pale green, blue, and emerald illustrate. One mineral water bottle had contained the familiar Congress Water, from Saratoga Springs, New York.

At Traveler's Rest two small medicinal bottles were recovered, both exhibiting pontil scars at the base and one blown in a mold in order to emboss the raised lettering: "Essence of Peppermint Patent, and Menthe", presumably a toothache cure or perhaps a perfume. The other small bottle still contained particles of another methel silicilate, oil of wintergreen, a rheumatism pain killer. A fragment of the sunburst decoration of an olive green flask or decanter was also found, a design typical of the early nineteenth century. Several lettered panel "patent" medicine bottles were recovered at Fort Jackson, many of which were bottled by local drug firms. All were blown in two piece molds with the lips hand tooled.

Generally speaking, most of the Jackson bottles were blown in two piece molds, had raised letters on the body, did not have company or mold numbers on the base, had mold marks running up the neck to the applied lip, had either a flat medicinal, double ridged string rim or blob type lip form, and were generally of pale green glass.

The climatic and soil conditions at Traveler's Rest were not conducive for the preservation of organic material, but, as mentioned earlier, the permanent moisture of the Jackson vaults was such as to preserve leather, wood and even cloth. Several parts of barrels, kegs and cranks were found.

Obviously, the artifact collections from these two sites are not all inclusive nor is it fair, by any means, to treat material from an historic frontier site and a military fort site as typical of each period. Nor is it, for example, a new discovery that graniteware replaced pearlware as the common pottery type in the period ca. 1865-1875, or that American pottery companies rivaled the English by ca. 1875, or that molded bottles replaced free blown bottles by the same period. However, the type transitions as illustrated by the two tightly dated artifact collections from Traveler's Rest and Fort Jackson at least substantiate the facts and dates generally suggested by historical records and collectors. Moreover, it goes without saying that the work in Georgia, 1968, furnishes at least a regionally valid chronology of some ceramics, metal and glass artifact types for at least a period of about twenty years of the nineteenth century upon which to continue to build our knowledge of the effects of time on certain artifacts. Whether or not these local conclusions hold true on comparable nineteenth century sites throughout the southeast and other sections of the country will depend upon our solving the ever present problem of publication, and how thoroughly we, as archaeologists, explain our contexts and research our artifacts. If these problems are overcome, perhaps we can begin to make some order out of the chaotic nineteenth century, which might not be quite so "overwhelming" as others have suggested.



Fig. 1. Cross section through the Traveler's Rest storage pit showing: A. hearth foundation, B. chimney destruction debris, C. occupation layer.

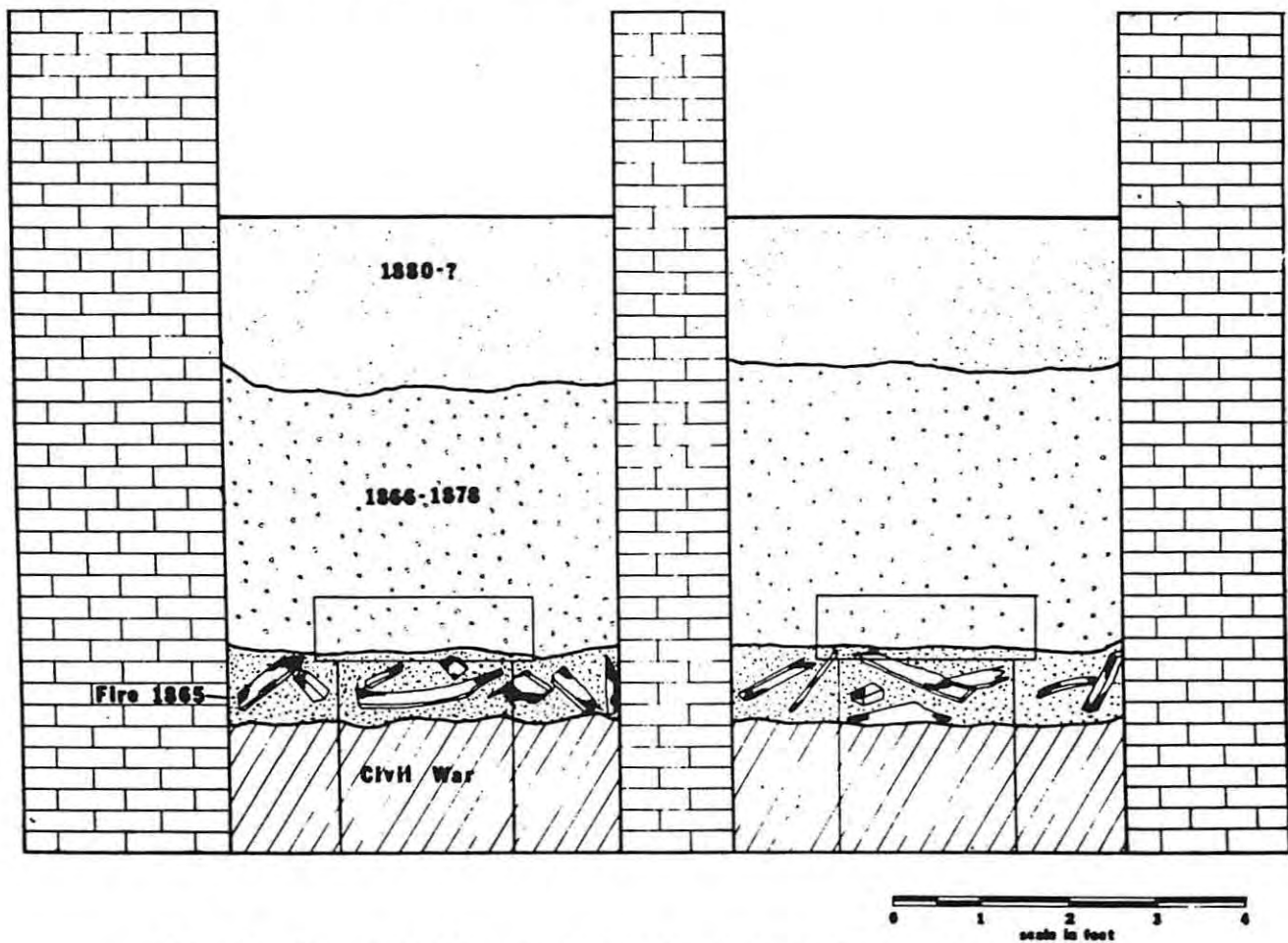


Fig. 2. Cross section through the Fort Jackson privy vaults.

- Fig. 3. Examples of blue and green shelledged pearlware (TR)*.
- Fig. 4. Cobalt blue and polychrome hand painted pearlware (TR).
- Fig. 5. Graniteware objects of the period ca. 1866-78 (Ft. J.)**.
- Fig. 6. Stoneware jug made at Trenton, S. C. (Ft. J.).
- Fig. 7. American pottery marks, left and center: Edwin Bennet, Baltimore: right: mark of company at Trenton, S. C. (See Fig. 6 above) (Ft. J.).

*Found at Traveler's Rest
**Found at Fort Jackson

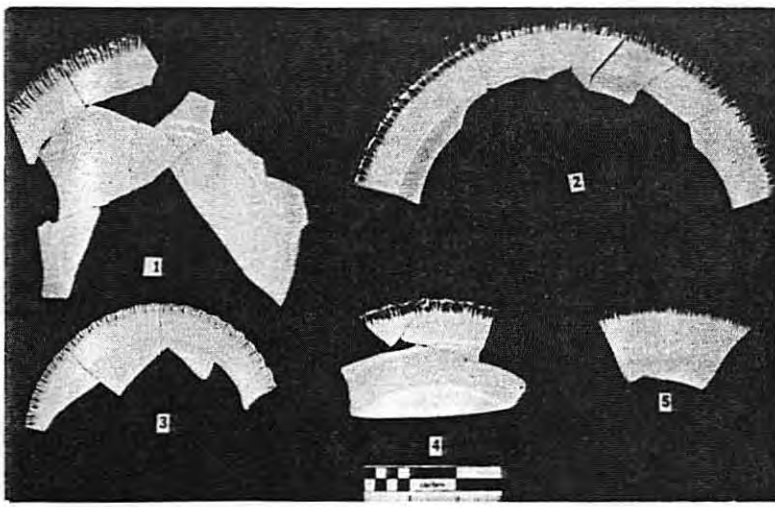


Fig. 3

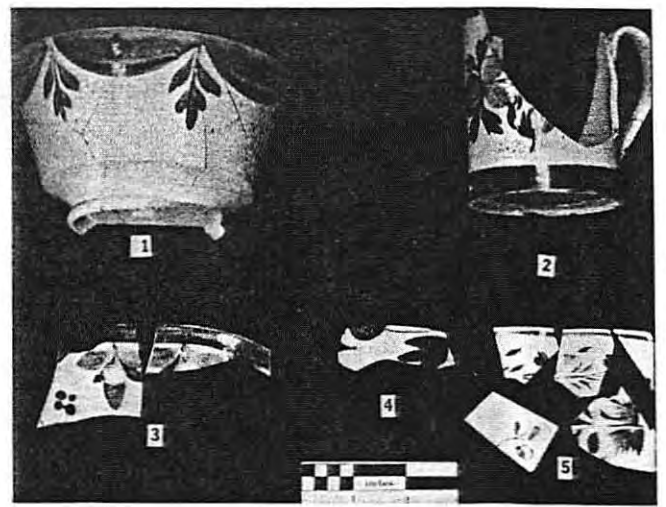


Fig. 4



Fig. 5

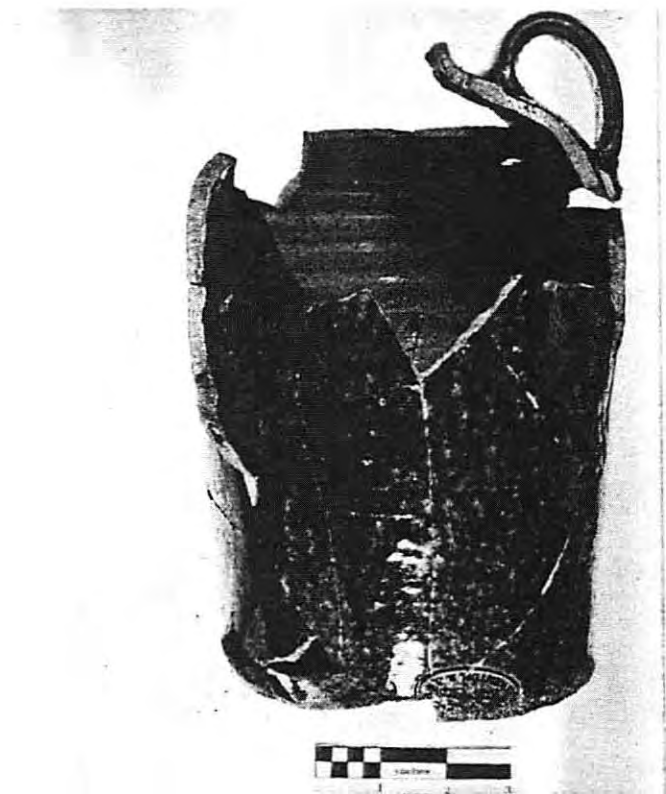


Fig. 6

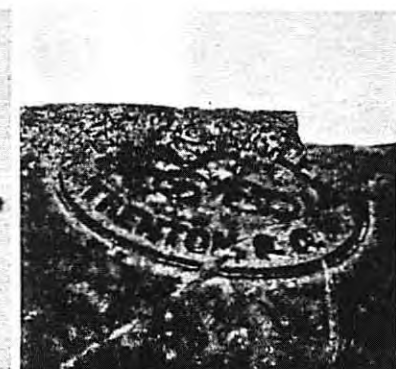


Fig. 7

- Fig. 8. Reedstem tobacco pipes (TR).
- Fig. 9. Kaolin tobacco pipes of the post Civil War era (Ft. J.).
- Fig. 10. Iron pintles and padlocks (TR).
- Fig. 11. Iron pintle and padlock (Ft. J.).
- Fig. 12. Early nineteenth century brass button types (TR).
- Fig. 13. Fragmented wine bottle of the early nineteenth century (TR).
- Fig. 14. Liquor bottles and flasks (Ft. J.).
- Fig. 15. Beer bottles ca. 1866-78 (Ft. J.).



Fig. 8

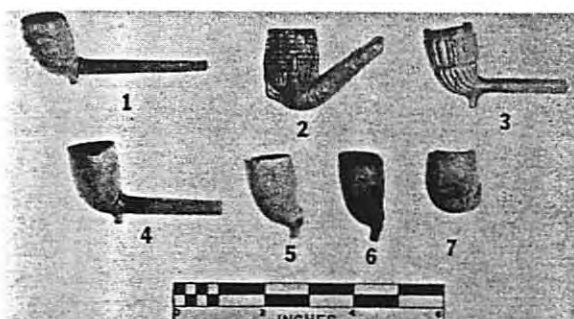


Fig. 9

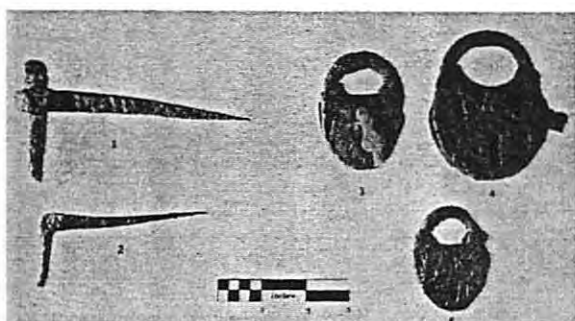


Fig. 10



Fig. 11

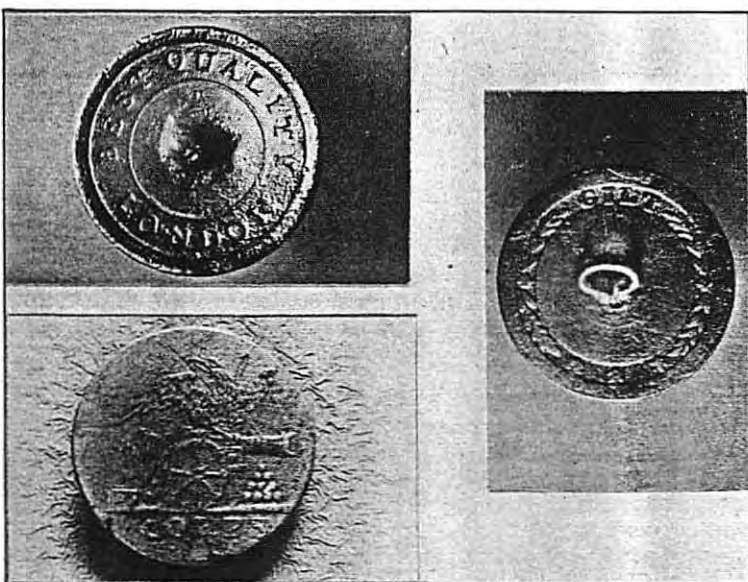


Fig. 12

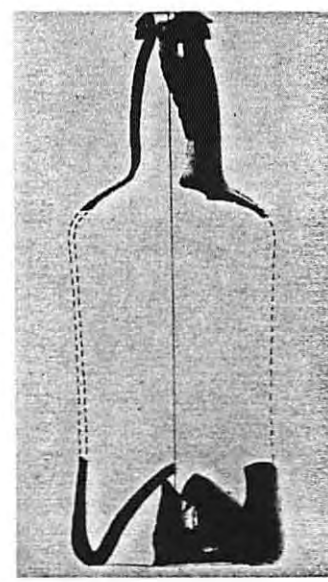


Fig. 13



Fig. 14



Fig. 15

NOTE ON THE BETHABARA, NORTH CAROLINA, TOBACCO PIPES

Iain C. Walker

Among the items found in the excavations at the Unitas Fratrum or "Moravian" settlement at Bethabara, North Carolina, those which interested this writer the most were the clay tobacco pipes. Unlike the conventional long-stemmed white clay tobacco pipes made of ball clay (not, as is frequently stated in North American literature, kaolin) the Bethabara examples were pottery pipes designed to take a reed or similar stem. They were thus made only with a bowl and a stub stem. In discussing these pipes the excavator rightly noted^{1, 2} that their discovery at Bethabara in contexts as early as the 1750's made this type of pipe earlier in date than had been supposed previously; the purpose of this note is to indicate some possible fields of research both on the origin and the subsequent development of this type of pipe.

The "Moravians" were a religious sect springing from the teachings of Jan Hus and included Bohemians, Germans, and Poles in their number as well as Moravians. In the early 1720's, after a century and a half of persecution in their homeland, the "Moravians" revived in Saxony, where many of them had settled, and established a religious community called Herrnhut near Zittau, where now the boundaries of Germany, Poland, and Czechoslovakia meet. This community attracted followers of the faith from Bohemia and in the 1730's many "Moravians" went as missionaries to many parts of the world. Those at Bethabara had arrived in 1752 and 1753 from Bethlehem, Pennsylvania, where they had settled in 1740, after attempting to settle in Savannah, Georgia, in 1735³.

The Bethabara pipes, as already noted, were pottery pipes. A minority were glazed, and many were anthropomorphic in design. From the almost complete absence of white clay pipe fragments found in the excavations it is clear that the Moravians preferred their own type of pipe. Conventional ball clay pipes were absorbent and thus give a relatively cool and mild smoke (though not compared to modern briar pipes); pottery pipes, especially glazed ones, would give a hot and strong smoke. Clearly this was what the Bethabara smokers wanted, and presumably this had been their tradition in central Europe. Gottfried Aust, the first potter to make these pipes at Bethabara, had come from Herrnhut⁴ where he had lived since 1732⁵ and it is reasonable to suppose he brought with him the type of pipe already favoured there (his name suggests a Bohemian origin, for Aust was the Germanized name for the town now known as Sezimovo Ústí, just east of Tábor, ca. 50 miles S of Prague; however, his family could have been living away from the Aust area for several generations).*

*[Editor's note] Aust was born April 5, 1722, at Heidersdorf in Silesia in the principality of Brieg. He apprenticed under Andr. Dober in Herrnhut in 1743.

As the Bethabara type of pipe and strong hot smoking do not belong to the English tradition which spread to the Netherlands at the beginning of the 17th century and thence to northern France and the Westerwald area of Germany probably ca. 1700, it seems likely there is a central European smoking tradition, the sophisticated version of which is represented by the famous porcelain pipe-bowls, and the study of which has been ignored in the preoccupation of contemplating the porcelain objets d'art.

The history of the porcelain pipe industry in Germany is obscure; according to some sources^{6, 7} porcelain pipe-bowls were being produced at Meissen in the first half of the 18th century, soon after the first successful manufacture of porcelain in Europe there ca. 1709 or 1710; another source gives a date of 1765 for the first porcelain pipes made at Meissen and notes that Meissen and Nymphenburg, a suburb of Munich, were the first centres to produce these bowls⁸. If porcelain pipe-bowls were indeed being produced in the first half of the 18th century their manufacture must have spread with the capture of Meissen during the Seven Years' War, 1756-63, which resulted in the spread of porcelain manufacture to other parts of Germany⁹. Obviously the bowls cannot be earlier than ca. 1710, and this agrees with the evidence of their shape which is derived from that of a conventional Dutch pipe which does not appear to be earlier than the first quarter of the 18th century.

In the 1870's porcelain pipe-bowl production at Ruhla and the surrounding area of Thuringia was 9,600,000 a year, the type being noted as "the favourite of the German peasant" which indicated that it had ceased to be an exclusive item by that time¹⁰. A contemporary - 1876 - British observer noted "The execrable china pipe is the mystery of the German. It has no absorption. It is a mere tobacco-still, condensing the fetid juices in the reservoir which must be frequently emptied and cleaned, or it is converted into a hubble-bubble of disgusting poison"¹¹. These sources emphasize the popularity of hot strong smoking in central Europe.

The custom of smoking was spread through central Europe by the English and Dutch mercenaries employed in the Thirty Years' War, 1618-48, which started in Bohemia^{12, 13}. It seems probable that soldiers wanting to smoke and unable to find English or Dutch pipes were prepared to make and use crude pottery ones and that from this the central European tradition evolved. In Switzerland, for example, lack of suitable clay and distance from areas of pipe manufacture appear to have resulted in the local production in the later 17th and the 18th centuries of metal pipes - with bowls showing Dutch and German features - which must have given an extremely hot and strong smoke¹⁴. Scarcity of tobacco may also have contributed to a desire for a strong smoke.

Pipes very similar to the Bethabara examples were used with wooden stems in Poland and made in Warsaw, apparently by the end of the 17th century¹⁵. Only two of the examples excavated bore any decoration - a simple flower - and none were anthropomorphic; they were glazed yellow or green. The method used to fire them - they were hung on clay "pins" attached to the outside of a vessel - is precisely that known from excavated remains to have been used at Bethabara. The pipes were made in bipartite clay or wooden moulds. There seems every reason to suppose these Polish finds represent the tradition which Aust inherited and brought to the New World. It is likely that an examination of the collections in the provincial museums in central Europe would unearth similar material ancestral both to the Bethabara material and the porcelain pipes of Europe.

Aust indeed may have set a style for later American-made pipes. Bethabara pottery was shipped to Virginia and South Carolina, and Aust was selling pipes to the Cherokee Indians as early as 1756, so clearly his products were popular. In the case of the pipes some of their popularity was no doubt due to their being both cheaper and easier and less fragile to ship in bulk than were the imported long-stemmed white clay pipes - talking of European examples, Duhamel du Monceau¹⁶ says the lower classes preferred them to the fragile white pipes because they could be put in the pocket without fear of breaking - but a taste for pottery pipes appears to have spread in North America during the 19th century, perhaps formed initially, as it probably was in central Europe, by difficulty in obtaining the more sophisticated white clay pipes. Aust worked at Bethabara from 1755 until 1771, when he moved to Salem where he continued his trade. He died in 1788 while in Pennsylvania. From 1786 to 1789 Rudolph Christ made pottery at Bethabara and on his taking over Aust's business in Salem, Gottlob Krause took his place at Bethabara. Krause continued at Bethabara until 1802, when John Buttner bought the business. Excavation of the Christ-Krause waster dump revealed pipes generally similar to those of Aust but slightly more evolved and all anthropomorphic. Inventory figures of the Salem pottery for 1789, 1790, 1805, 1807, and 1822 indicate quantities of unglazed pipe-bowls in stock. (In 1820 John Holland became the potter - he was there in 1822.) Many pipes were sent north, and in 1807 there are references to reed pipestems being sent to a John Jordon of Philadelphia. In 1789 a waggon-maker, Tycho Nissen, was allowed by an agreement between Christ and the Church to continue making pipe-bowls at Salem. Having previously made pipes for Aust, he was allowed by Christ to continue to do so for him. The Salem pottery continued until late in the 19th century, and appears to have produced pipe-bowls to the end. When it closed, much of its equipment found its way to the Wachovia Museum in Salem, but some went to journeymen potters who carried on the traditions, including that of pipemaking, for another generation¹⁷; indeed, a colleague of the writer bought basically similar though non-anthropomorphic pipe-bowls apparently of fireclay in Salem in 1967, and anthropomorphic pipes with a turban are still being made from an old Salem mould.

Unfortunately, virtually no work has been done on American-made pipes. A maker is recorded¹⁸ in Maine in 1882 using ball clay "from the very bed used by Glasgow pipe-makers" (Glasgow being the chief supplier of white clay tobacco-pipes in North America last century) and Montreal and to a lesser extent Detroit had a relatively important white clay pipe industry in the second half of last century¹⁹, but pipes generally produced in the United States appear to have been pottery pipes. In Massachusetts a Shaker community was making pottery pipes before 1800²⁰ which took wooden stems, and similar pipes were made in Maine apparently in the 1850's and in New Hampshire in the 1860's - from which later they were shipped to New York and other centres - by John Taber²¹. (It is not certain it was the same John Taber in each case, but it seems likely. His [or their] origins are not recorded - the similarity of the name to that of the Taborites, an early puritan Bohemian sect named after the town of Tábor founded by the Husites and already mentioned, is perhaps coincidence.) Stub-stemmed pottery pipes are in the Bucks County pottery exhibit at the Bucks County Historical Museum, Pennsylvania²² - presumably these date to the last century or to the beginning of this century, but they may represent a tradition handed down from the settlement of the area by people from various areas of central Europe, particularly from the Rhine valley, before 1700. Stub-stemmed pipes were being made in Ohio last century²³, an area where Bucks County Germans had settled²⁴. Pipes of this class were being made in the Pamplin area of Virginia by the Pamplin Pipe and Manufacturing Company, Inc., and by individual makers²⁵. The foundation date of the Pamplin Company was given as 1739 in advertising matter of the early 1940's, but better proof of this early a date has yet to be found²⁶ - clearly if it or other Pamplin makers were producing pottery pipes this early they would antedate Aust by 16 years. However, pipes of this type found in Virginia are noted as being most common during the period 1840-1900²⁷, and if, as seems likely, these were Pamplin products, an early date for the production of these pipes seems unlikely. (These Virginia pipes were red-bodied and glazed; the Pamplin pipes described by Thompson are red-bodied but no glaze is mentioned.) Pamplin pipes were shipped to other parts of the United States and also abroad²⁸.*

Pottery pipes with stub stems accompany the opening of the American south and west towards the middle of the 19th century²⁹, and it is possible this represents people taking with them the tradition of the south-eastern United States originated by Aust. Certainly these pipes were being made in Texas by the second half of last century³⁰. Further north and west, this class of pipe has been found on a number of 19th-century frontier sites - for example, in New Mexico³¹, Oklahoma³²,

* An article on Pamplin manufacturing equipment is to appear in the next issue of the Quarterly Bulletin, Archeological Society of Virginia (information from E. T. Heite, Virginia Historic Landmarks Commission). Mr. Heite also informs me that other work is being done at present on Pamplin pipes.

North and South Dakota³³⁻³⁶, Minnesota³⁷, Wyoming³⁸, and Washington³⁹, as well as in Florida⁴⁰, but well-dated contexts appear to be lacking save for examples from Old Sacramento City, California⁴¹, where they are datable to 1846-52 and specifically to the latter year, and from Fort Pierce, Florida⁴², where they probably date to the period 1838-42. They did not occur at Kipp's Post in North Dakota occupied ca. 1826-30^{43,44}, which might suggest they were not yet common at that time at least in the west, but they also did not occur at Johnny Ward's ranch in Arizona occupied throughout the second half of last century⁴⁵, while occurring elsewhere in southern Arizona at this period⁴⁶. (An element of confusion is added here by the production last century of elaborate, and often superlative, effigy pipes with stub stems in Europe, particularly in France and Belgium. These were probably not particularly common in North America, but some designed specifically for the United States market are known⁴⁷. However, these pipes appear never to have been made of earthenware, but it is possible they did inspire American manufacturers to produce cheap imitations.)

Simple moulds of lead encased in wood were used in the Pamplin area to make these pipes by very primitive hand production, but the use of machinery was also known⁴⁸. Aust used brass and lead moulds and a 1766 inventory listed a "tobacco pipe press"²; certainly latterly the Salem industry used a press to assist in the manufacture of its pipes⁴⁹.

The whole field of 19th-century American pipes requires study, but it is a tempting hypothesis that their style was brought by Aust from central Europe and popularized by the excellence of his products, and that he established a trend which still survives today⁵⁰.

POSTSCRIPT

Since this was written, E. F. Heite's note "Pipe Industry History Reflected in Tools" has appeared (Quarterly Bulletin, Archeological Society of Virginia, Vol. 24, No. 2 (December 1969), pp. 118-9 and cover photograph.) Heite notes that the factory production of pipes at Pamplin commenced only in 1870. John W. Walker of the National Parks Service, South-East Archaeological Centre at Macon, Georgia, also indicated a much later date for pipe production at Pamplin than has been generally asserted (in conversation, Eleventh Annual Conference on Historic Site Archaeology at Columbia, South Carolina, October 1970) - Walker's research may be published at a later date. John Ewers of the Smithsonian in Washington also has some information on the Pamplin pipe-factory.

However, a fairly crude stub-stemmed pipe with primitive incised stem decoration has recently been found in a rubbish-dump in coastal Virginia with material deposited ca. 1730; it appears to have been made with the help of metal tools (E. F. Heite, "An Eighteenth-Century Reed-Stem Pipe from Lancaster County", Quarterly Bulletin, Archeological Society of Virginia, Vol. 24, No. 4 (June 1970), p. 226 and cover photograph).

Heite notes this is one of the earliest known contexts in Virginia for such a pipe; However, so far there seems no compelling evidence to indicate the pipe was made by Europeans. Its presence in this context, nevertheless, would indicate a tradition of this pipe type this early. A full report on the context of this find is to be prepared.

In another recent article, E. J. Lenik notes that brown clay bowls designed to take stub stems have been found on historical sites in northern New Jersey ("Nineteenth Century Tobacco Pipes (A Preliminary Study)", Quarterly Bulletin, Archeological Society of Virginia, Vol. 24, No. 3 (March 1970), p. 175).

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EXCAVATING THE FORTIFIED AREA OF THE 1670 SITE
OF CHARLES TOWNE, SOUTH CAROLINA

Stanley South

In 1664, a settlement known as Charles Towne was begun on the Cape Fear River in North Carolina. After three years the effort was abandoned. Within two years after the Cape Fear venture had failed, a new expedition was planned through the leadership of Lord Anthony Ashley Cooper. This effort was aimed at a settlement at Port Royal on the Carolina coast, but at Bull Bay the cassica of the "Kayawah" Indians came aboard and convinced Governor Sayle that a better prospect lay at a site on the south bank of the Kiawah River, later to be known as the Ashley. This arrangement would allow supplies to be brought by the Kiawah Indians to the infant settlement and in turn the settlement would provide protection for the Kiawah against their enemies.¹

At first the community was known as "Albemarle poynt at Kyawaw," but this was soon changed to Charles Towne. After their first landing at Albemarle Point in April, 1670, they immediately began to fortify their position, both against possible Spanish attack and unfriendly Indians. William Owen, one of the leaders of the colony, provided us with a description of the site.

...we haue made choise for ye better, hauing pitcht on a pointe defended by ye maine riuer with a brooke on ye one side, and inaccessible Marshe on ye other wch att high tides is euer ouerflowne: ioyning itself to ye mainland in a small neck not exceeding fiftie yards which now is pallizadoed, and with a verye small charg might be made Impregnable: for neither by water on ye one side nor by land on ye other cann ye enemie make any considerable attack but yt a handfull of men may defend with securietie, if this neck of land would be seuered from the Continent.²

In view of the archaeological evidence, this reference to a neck with a width of fifty yards "which now is pallizadoed" is of particular interest, and this point will be discussed later.

In 1671, John Culpeper made a map of the Ashley River area on which he showed three symbolic house structures and marked the site of Charles Towne.³ By correlating this draft of 1671 with the United States Geological Survey maps of the area, it is possible to pinpoint the location of Charles Towne on present day terrain. By so doing it can be seen that the Charles Towne site is located on the point of land known as "Old Towne Plantation," a tract of land acquired in March, 1969, by the South Carolina Tricentennial Commission for development as an historical park commemorating the first settlement of South Carolina.

The Charles Towne settlement was designed for planting and trade, but to successfully carry these into effect a protection against Spanish attack was necessary. Before leaving England for the Carolina venture in 1669, Joseph West was supplied with:

...four iron demi-culverin and eight sacres, with ship carriages, ladles, sponges and linstocks & 12 round of shot for each....⁴

The colonists were instructed to build a fort for the protection of the town, in which were to be placed the artillery pieces. Fortification was begun shortly after their arrival and several guns were mounted, although the carriages had been lost in a vessel that was wrecked by a storm on the crossing.⁵ A stimulus for further fortification was provided when it was learned that three Spanish vessels with infantry had been sent out from St. Augustine to launch an attack against the settlement at San Jorge, the name the Spaniards applied to the Charles Towne settlement. On August 6 and 7, 1670, a hurricane hit the Carolina coast and forced the Spanish expedition against Charles Towne to be called off, the storm battered vessels returning to St. Augustine.⁶ The attempted assault on the Albemarle Point settlement resulted in an extreme effort at fortification "as strong as they could," and by March of 1671, it was reported that:

We have with much adoe, our people being weake by reason of scarcity of provisions, pallisadoed about 9: Acres of land, being a point, whereon we first set downe for our better security, and mounted seaven great Gunns, all the other carriages haveing been lost with the ship Port Royall.⁷

Although their expedition against the settlement of the English had failed in 1670, the Spaniards had not forgotten their unwanted neighbors. The governor of St. Augustine, Don Manuel de Cendoya, sent a spy named Camunas (who could speak ten Indian languages) to the settlement on Albemarle Point to find out what he could about the details of the fortification at the English town.

Camunas traveled overland to the English settlement, assisted by some men and a canoe. At the Indian village of Osao he met a female cassica who had been at St. Augustine, who insisted on accompanying him to within a short distance of the English town. He was met by six Englishmen, "all with military insignia," who escorted him to the village and to the governor's house. The governor received him cordially, and Camunas presented a packet of letters, and was shown the garden plots while the letters were being read. After this tour of the vegetable garden the governor offered wine in a silver cup, and toasts were exchanged for the kings of England and Spain.

While enjoying the hospitality of the English governor, Camunas was observing all around him with more than a tourist's eye, and later reported on July 12, 1672. He stated that at the entrance to the village on the land side there was a strong house of wood, and that within this wooden fort fifty men were stationed with an infantry captain in charge, and in the house were many firearms, shot-guns and naked cutlasses. After passing this

wooden fort he saw about ninety houses in the village, without any formal streets. After leaving the village:

...along the edge of the river some houses continue, all of wood & disposed with much regularity, until one comes near the Castle [fortification] which also is of wood made into a sort of wall of heavy logs & its height & elevation is about two and a half varas [7 feet] with twenty-eight pieces of artillery of iron & bronze, twelve of which are pointed toward the river & the port by which the ships enter, and the rest, from their low embrasure, toward different points on the mainland. And inside of this fortification there are some lodgings and others of the same sort outside of it which, as he was informed, were built at first when they began to settle for fear of the Indians.⁸

The "sort of wall of heavy logs" seven feet high would appear to be a palisade, inside of which was twelve pieces of artillery pointing toward the river and port of entry, with the rest "from their low embrasure," pointing toward various points along the mainland. The "low embrasure" would appear to be an earth embrasure accompanying the palisade along the land side of the fortified area. Inside of this fortified area there were only "some lodgings...which...were built at first when they began to settle for fear of the Indians," Thus it becomes quite clear that the fortified area described here by Camunas as having only some lodgings, was not what he considered as the village of Charles Towne in the summer of 1672, the settlement proper being located some distance away up the road "along the edge of the river," from which he had reported some ninety wooden houses.

Further description of this fortified area of Charles Towne comes from four Englishmen who had lived at the Ashley River settlement and who, in February, 1674, fled to St. Augustine as refugees to the Spanish, saying that Charles Towne suffered from lack of supplies, and that they had been mistreated there. They reported that the settlement contained about one hundred men, that there was no paid infantry, and that there was a fort built with stakes and fascines.⁹ The "stakes" were likely the same palisade "wall of heavy logs" described by Camunas, and the fascines would imply earthen embankments faced with bundles of sticks. Three of the refugees stated that the fort consisted of a platform with stakes and fascines, or stockade and fascines, "which was practically demolished, and which contained about thirty pieces of artillery," with about ten pieces mounted on the platform. This description verifies the presence of a stockade, and of a fascine covered embankment, with the additional reference to a "platform" on which the artillery pieces were mounted. The platform could have had reference to artillery pieces on ship carriages mounted on individual platform, behind a fascine covered parapet, a typical situation. It could possibly also apply to a cribbed log platform of some sort on which the artillery pieces were mounted.¹⁰ A Spanish document dated July 26, 1671, stated that at San Jorge there was a wooden fortress near the water with ten pieces of artillery aimed toward the port, with seven of these set on land and the others on beams on the ground. These beams were very likely

the wooden gun platforms for artillery with ship carriages.¹¹

We know that by 1674, the original palisade at the fortified area of Charles Towne was "practically demolished," and on February 9, of that year the Grand Council at Charles Towne advised:

...upon the erecting of a new fortification about Charles Towne
It is resolved that Capt Stephen Bull be present at the Council
upon Friday next with his surveying instruments to run the line
of the said fortification as shall them be advised.¹²

The fact that surveying instruments were necessary to run the line of the new fortification would indicate that it was located in a different place from the original.

The Spanish never got around to attacking the original settlement, and it continued to grow and spread out along the banks of the Ashley River. Throughout the 1670's people moved from the area of the original settlement site to Oyster Point between the Ashley and the Cooper Rivers, where a town was fast developing. By 1679, the new Charles Towne was declared the port town, and by the following year the old site was referred to as, "Kaiawah sometimes called Charles Towne." From this time on, Oyster Point was to be known as Charles Towne, and the original landing site came to be known as "Old Town Plantation."¹³

A new Spanish scare occurred in the summer of 1682, as the word reached the new Charles Towne that 800 Spaniards were marching toward the town from St. Augustine. The Council met and ordered that 20 great guns:

...that lay at a place where the town was first designed to be made, to be brought to Charls Town...¹⁴

If the twenty guns were removed to the new town at this time there were probably some eight or ten remaining. In a document dated November 18, 1685, the Lords Proprietor stated:

...that there are divers peeces of our Cannon that lye unmounted & useless at old Charles Town And haveing taken into our consideration That Stewarts Town at Port Royall is the Frontier of ye Whole Settlement towards ye Spainard and most lyable to be hurt by them whenever they Shall be disposed to disturb us
Wee doe therefore Order That you deliver Five of ye aforesaid Peeces of Cannon to...Stewards Town or Some other Town in Port-Royall in Carolina and there to mount ye same for ye safety thereof...¹⁵

This accounts for almost all the artillery known to have been in the Charles Towne fort, and with the removal of the artillery the fifteen year recorded history of the fort comes to a close. Between 1694 and 1697, 760 acres of land known as "Old Town Plantation" was granted to James Le Sade. This included the site of the original Charles Towne and fort. From that time to the present, the property has carried the name "Old Town Plantation."¹⁶

ARCHAEOLOGY

In November and December of 1968, John Combes and I conducted a five week survey of the tip end of Albemarle Point where the fortification for Charles Towne was thought to have been. No evidence of unusual topographical features could be seen in the pine woods now covering the site, except what appeared to be an old roadbed at the edge of the marsh on the west side of the peninsula, and a similar depression at the edge of the march on the east side of the point. These features were not immediately recognized as the remnants of the seventeenth century fortification ditch and parapet though speculation as to this possibility was made. Surface material over the site was limited to nineteenth century ceramics and glass, with only two sherds of seventeenth century stoneware being found on the surface during the entire archaeological search.

The design of the survey was to locate any evidence remaining of the original settlement at Charles Towne in the 1670's. The project was sponsored by the South Carolina Tricentennial Commission through the office of the Institute of Archeology and Anthropology at the University of South Carolina. Preliminary work had been undertaken by an untrained individual who had attempted to locate evidence of the palisade thought to have been "across" the fifty year neck of land separating the tip of Albemarle Point from the mainland. A number of ditches had been found, and fortunately these had not been excavated, but only revealed at the subsoil level, allowing re-cleaning and plotting to be carried out with no loss in data recovery. Exploratory trenches were cut at various places on the point, with a ten foot wide trench running down the center of the peninsula. The exploratory work was carried out in an area of some 16 acres, mostly of woods, and the base line method of direct transit readings was used to record all data so revealed.

In many places throughout the site a series of parallel ditches were revealed, from six to eight feet apart, and from twelve to eighteen inches in width. Hardly a trench could be cut without encountering these ditches. An agricultural practice was indicated, but a puzzling fact was that the ditches were found both on the high ground, and in the low marshy areas, and it was difficult to imagine what crop could be successfully grown in both types of soil. On the suspicion that the ditches may represent vineyards, research into vineyard culture was undertaken, and a book on the art of the planting of vineyards, published in 1772, was found. This source stated that vineyards in South Carolina at that time were planted in straight parallel furrows, each of which was $1\frac{1}{2}$ feet broad, and $1\frac{1}{2}$ feet in depth, and spaced six feet apart, and that the vines would flourish in the low marshy ground as well as the higher terrain. The culture of the vine was said at that time to be well known at Charles Towne.¹⁷ The report of the Spanish spy Camunas stated that each lot in Charles Towne had a trellis for grapes, providing evidence that the vineyard ditches could date anywhere from 1672 to 1772 and later.¹⁸

One of these ditches when excavated produced a quantity of burnished Indian pottery, shell tempered red filmed Indian pottery (from the West Indies),

wrought nails, oyster shell midden, stamped Indian pottery, and a kaolin pipe of seventeenth century form, impressed on the heel with the letters "EB". This may be the mark of Edward Battle, who received his freedom as a pipe-maker in 1660. From the contents of the vineyard ditches, it appeared that they were seventeenth century in origin. Several midden pits were also found that contained the same cultural materials, "EB" pipes, wrought nails, Indian pottery and midden.

Pipes marked "EB" have been found in an Indian grave (in the late nineteenth century) by Mr. S. L. Frey of Palatine Bridge, New York;¹⁹ and by Mr. P. C. Hiller of Conestoga, Pennsylvania, in Lancaster County; and by Mr. Lott Van De Water in Hempstead, Long Island, while digging a well.²⁰ They are not thought to have been made in Hull, England, though one was found in excavations there.²¹ In discussing Phillip Edwards (a Bristol pipemaker in 1649), John Pritchard says that he appears to have taught three lads the pipe making trade, one of whom was Edward Battle, who took his freedom in 1660.²² Some "EB" pipes were also found in excavations at Jamestown, Virginia.²³

In the area of the fifty yard neck joining the tip of Albemarle Point to the mainland, a series of parallel vineyard ditches were found, but no evidence of a palisade across the neck was seen. Rather, a large ditch was found extending up the center of the high ground of the neck instead of across it as had been traditionally thought from the reference of the neck "which now is pallizadoed." This ditch, measuring six feet wide and from two to three feet deep was found to extend to the marsh on the east, and tests were made in the marsh itself at the base of the high ground of the peninsula, and the ditch was located here also, indicating that it was once a water filled ditch or moat for 180 feet, clearly revealing its function as a fortification ditch, rather than as a property line or for drainage. A ditch at this position along the edge of Albemarle Point would have provided protection against a possible attack across the marsh from high ground to the north. From the edge of the marsh, the ditch continued for 810 feet where it was found to join another ditch, slightly smaller, which angled sharply toward the south, producing an acute angle at the junction of these ditches. Along this north ditch two re-entered angles were apparently designed to allow for cross-fire protection along the face of the fortification. On Culpeper's map of 1671, a property line for two and four acre lots joining Charles Towne on the north was shown. This line was seen to have an angle of 123°. The relationship between the north fortification ditch along two of its sides was found to also have an angle of 123°. This correlation between the 1671 map and the archaeologically revealed ditch provided evidence for dating the ditch as having been dug prior to 1671, and established it as the north property line and fortification ditch for the Charles Towne settlement mentioned as having been completed by September, 1670.

The west fortification ditch was dug prior to the north ditch which joined it, although this west ditch was not shown as a line on the 1671 map. This fact was revealed in the manner in which the north ditch angled sharply immediately before joining the west ditch, and became shallower, indicating that the west ditch was in existence when the north ditch was dug; thus both ditches were in existence by September, 1670. The west ditch continued in a

straight line to the marsh on the creek side of the peninsula, being 765 feet long. The total length of this fortification ditch along the land face of Albemarle Point was 1755 feet. The "low embrasure" mentioned by Camunas in 1672, behind which some sixteen artillery pieces were mounted, was no doubt, the embankment accompanying the north and west ditches here described.

At several intervals inside the north fortification ditch of 1670, at a distance of five and one-half feet from the center, a small ditch was found paralleling the fortification ditch. This was a very shallow ditch, portions of which had been destroyed by the plow. The position of this ditch in relation to the fortification ditch was such that it fell directly beneath the center of the parapet which would have accompanied the fortification ditch. This parapet position is based on the assumption that the parapet would have been composed of the same amount of dirt that was thrown from the ditch. This parallel, accompanying ditch, therefore, apparently represents the bottom of a palisade ditch. A few postholes in the ditch and in alignment with it tend to support this interpretation.

Since we know that the north fortification ditch and palisade was constructed by September, 1670, as well as the west ditch, we may well ask why Culpeper did not show the west fortification ditch on his 1671 map as well as the north ditch. One reason is the fact that with the north ditch he was showing the relationship to two and four acre tracts of land lying just outside the town fortification, whereas on the west, "Behind the towne," he stated that Governor Sayle had set aside sixteen acres for himself, and four acres for a churchyard. (S.P., V, frontispiece). From this we see that Governor Sayle's sixteen acres joined the town, and since two and four acre lots were located on the town border on the north, Governor Sayle's property joined the town on the west. Since the governor's property and the churchyard were no doubt part of the town plan, the fortification ditch separating them may not have been shown by Culpeper for this reason.

Another reason Culpeper may not have shown the west fortification ditch line on his 1671 map may relate to Governor Sayle's death, and the subsequent building by Governor West of the plantation for the Lords Proprietor across Old Town Creek from Albemarle Point. Sayle died on March 4, 1671, and by the 21st, Governor West reported to Lord Ashley on the palisade enclosed area he was completing for the residence of the governor at that site. (S.P., V, 119, 297, 421; S.C.H.G.M., XVI, No. 2, 51). It may well have been that upon Sayle's death West knew that the sixteen acres previously used by Sayle as a plantation, could now be utilized as a part of the Charles Towne settlement. We know that Culpeper's map was being made prior to Sayle's death because of the mention of the sixteen and four acre tracts set aside by him. Also, the map indicates land owned by Capt. Joseph West, and inserted above West's name is the note "our present governour." This note tends to indicate that the map was still being worked on when Governor Sayle died and West took over as governor. It may be that West knew that the settlement would be expanded to include the sixteen acre tract previously held by Sayle, and may have instructed John Culpeper not to indicate the west fortification line on the map since it would no longer mark the boundary of the settlement. This then, may account for why this fortification line of 1670 was not shown on the Culpeper map of 1671.

Archeology tends to support this interpretation through the virtual absence of artifacts in the west fortification ditch as compared to the east ditch. The width of the west ditch is narrow, with virtually no indication of erosion, whereas the east ditch is wider and shows signs of being left open for some time, as well as containing artifacts dating to the second decade of the eighteenth century. Archeologically then, there is evidence that the west ditch was open for a very short time after 1670.

Events later in the year point in this direction also. Lord Ashley instructed in December, 1671, that the town be made more orderly and convenient (S. P., V, 360), and within six months the Council passed an "Act for the uniforme building of Charles Towne." (S.P., V, 393). And as a result, in July, 1672, people owning lots in Charles Towne came before the Grand Council and turned them in, and were issued new ones according to the new plan of the town. (S.P., V, 408). With sixteen acres coming available in 1671 with the death of Governor Sayle and the completion of the governor's residence across from Albemarle Point, plus the urging of Lord Ashley to re-design the town layout, plus the archeological data indicating a short time period for the west fortification ditch standing open, there is strong evidence for the expansion of the settlement toward the northwest, outside the original fortification ditch limits. In such an event the west fortification would only serve as an artificial barrier to free movement in this area, and would have been allowed to fill up, or even encouraged to do so after 1672. By 1674 a witness stated that the palisades of the fort were practically demolished (S.C.H.G.M. XXXVII, No. 3, 96), and in February of that year the Grand Council requested that the surveyor bring his instruments to run the line of "a new fortification about Charles Towne." (S.P., V, 462). This new fortification was undoubtedly further to the north than the original west fortification, and is yet to be located archeologically. It appears quite certain, therefore, that the west fortification ditch as discovered in this project, represents the Charles Towne fortification from 1670 to around 1672, and certainly no later than 1674. The north fortification ditch and palisade, however, was apparently still used as a part of the new 1674 fortification, and was still standing open as late as the second decade of the eighteenth century, judging from wine bottle fragments, tobacco pipes and other objects dating from that period found in the upper part of the ditch fill. By following the fortification ditch in its continued extension toward the north, future archeology could very likely reveal the location of the fortification and palisade constructed in the new survey of 1674.

Regarding the sixteen acres of Governor Sayle, and the four acres set aside for a church yard, it has been found that the land on Albemarle Point lying to the west of the west fortification ditch contains twenty acres of high ground bordered by marsh and streams. This twenty acres is broken into two natural tracts by a low marshy area extending itself toward the west fortification ditch from the Old Town Creek marsh. The southernmost tract is the highest point on Albemarle Point, presenting a beautiful view of Old Town Creek and the site of the governor's plantation across the creek. This smaller tract is four acres in extent, and is likely the same four acres

set aside by Governor Sayle for a churchyard. Extensive slot trenching here failed to produce evidence of graves or of a seventeenth century structure of any kind. The larger naturally contoured tract contains sixteen acres, and is no doubt, the tract set aside for Governor Sayle's plantation. This site today is a garden, and includes the home of Dr. J. I. Waring. Archeology on the high ridge to the west of the Waring home may well reveal the evidence for the Charles Towne houses not found inside the fortification of 1670 to 1672, and may well discover the ruin of Governor Sayle's home. This is also the site of the nineteenth century "Negro Settlement" shown on the 1836 map of the Jonathan Lucas Plantation. Excavation here should produce some interesting results, both from the seventeenth century Charles Towne settlement, as well as data for the interpretation of Black history as it relates to the plantation economy as revealed in the ruin of the eighteenth century Horry-Lucas plantation house. Hopefully such archeology will someday be undertaken.

In summary of the data revealed by the excavation of the ditch found along the land side of the tip of Albemarle Point, it appears that shortly after the colonists arrived at Albemarle Point in 1670, they began excavation of a ditch which would allow protection from possible Indian attack in this direction. Rather than placing a simple palisade across the fifty yard neck of land separating the tip of Albemarle Point from the mainland, they ran a fortification ditch with parapet and accompanying palisade in such a manner as to offer the maximum protection against attack by land. To have simply run a palisade across the fifty yard neck would have left the entire west side of the tip of Albemarle Point open to attack, for then a shallow marsh twenty to thirty feet wide would have been the only barrier against attack, and was hardly sufficient. The colonists were wise enough to recognize this obvious fact, and rather than palisade across the neck, they ditched and palisaded up the length of the neck, thus encompassing both of the tips of Albemarle Point, and eliminating the possibility of attack on one point from the other. This seems a clear and obvious interpretation based on the archeological data and the military necessities presented by a defense of Albemarle Point against a land attack. However, it also vividly illustrates how misleading the written word can be when used alone without corroborating archeological evidence. The reference to, "a small neck not exceeding fifty yards which now is pallizadoed," would logically be, and has always been interpreted to mean, that a palisade extended across this neck. Only archeology could reveal that the reference was describing a palisade running up the length of the neck, which was found to have been the case. In such cases archeology provides an absolute check against continuing the historical bias resulting from a semantic trap. Some historians continue to argue for a palisade across the neck in the face of archeological evidence to the contrary, preferring the fickle word to the concrete reality.

The north fortification ditch was dug with two reentered angles so as to provide a crossfire cover in case of attack. This ditch also served as a property line for the area inside the fortification as well as for the two and four acre plots just outside this ditch. The west fortification ditch had no reentered angle, which is understandable in view of the fact

that considerable protection from marshes was afforded along this side of the area. A deed record of May 20, 1672, mentions that a three pole lot was bordered, "Westerly on the Towne fortificac'on or Palisadoes...",²⁴ apparently a reference to the west fortification ditch found archeologically. This reference is the last we have to the west fortification until a new palisade was constructed in 1674. The fortification along the land face of Albemarle Point as discovered archeologically and interpreted in an explanatory exhibit on the site consisting of ditch, parapet and palisade, represents the first fortification of this area by the Charles Towne settlers, between 1670 and 1674.

Near the tip of Albemarle Point where the deep water channel of Old Town Creek touched the high ground, a ten foot wide exploratory trench crossed a disturbed area seventeen feet wide. At first this was thought to be a cellar outline, but upon excavation this disturbed area proved to be a ditch seven feet deep and five feet wide at the bottom. In the fill of this section of the ditch an "EB" pipe of seventeenth century form was found. Exploratory trenches were cut following this ditch, and it was found that it extended from the marsh on the east side of Albemarle Point to the march on the west side, some six hundred feet. Near the center was a reentered angle, allowing for a crossfire against the entire area of the tip of the point. This feature was apparently a major fortification ditch, much more impressive than that found along the land face of the peninsula.

With the discovery of these fortification ditches, the preliminary five week exploratory project was brought to a close. A report was prepared on this phase of the project, and recommendations were made for further archeological work to begin on the first of April, 1969. This extended project was designed to excavate the fortification ditches both on the water side and the land side of Albemarle Point, with the ditches to remain open, and the dirt taken from them placed in position where the original parapet had been. The palisade along the land side of the point was to be replaced in the position indicated by the ditch paralleling the fortification ditch. This explanatory exhibit has now been completed.

The plowed soil zone was stripped from the area of the fort ditch by using a front loader, after the trees were cut and the stumps removed. The chevron shaped "V" ditch was found to form a smaller "V" near the creek end, clearly indicating where the entrance into the fort was located, and revealing that vessels were unloaded near this entrance and goods passed through it into the fort.

During excavation of the contents of the ditch, it was necessary to sift much of the dirt through window screen in order to recover the small lead shot and blue and white seed beads found by the hundreds. Some areas, however, were so devoid of any type of artifact that no sifting was carried out. The profiles of the ditch revealed a thin humus layer at the bottom apparently representing the first accumulation of humus, probably leaves, that were deposited after the ditch was excavated in 1670. Above this were alternating layers of water laid sand and clay containing no artifacts, and humus filled areas representing periods

of stabilization of the mound, during which leaves, weeds and other organic matter were being deposited as the ditch filled up through the years. The top of the filled ditch, in the area just below the plowed soil zone, a dark humus layer was seen, and it was this layer that contained a quantity of seventeenth century pipes, musket balls, straight pins, a Louis "XIIII" jetton, seed beads, Delft, and Bellarmine jug fragments.

Immediately in front of this ditch, between the point of the re-entered angle and the "V" of the entranceway, a circular ditch was found. This ditch was from ten to twelve feet wide and was in the shape of a fan, the center orienting at a 90° angle off the fort ditch and measuring 65 feet from one side to the other. Ten feet inside this large ditch was a smaller one measuring from six inches to one foot wide, also in the shape of a fan. In the center of the area inside this ditch was a large posthole 2½ feet across, and beside this hole was abundant evidence of the presence of many fires. These fires had baked the ground to a black and red brick-like hardness. In the ash and burned soil of this hearth area, lead casting sprues for swan shot were found, along with a large quantity of wrought nails, many bent, indicating that the wood for the fires was apparently being salvaged from some razed structure. Several small "L" shaped wire pins were also found in the ashes of this hearth. A brass drawer handle and fragments of an escutcheon plate were found in the dirt taken from the posthole. As excavation of this feature progressed, it became apparent that these ditches represented an artillery redoubt in front of the main body of the fort. The small ditch represented the position of vertical timbers no more than six inches wide, and the outer ditch was a fortification ditch from which dirt was obtained for an embankment between the two ditches. The central posthole was apparently designed for a post no larger than eighteen inches and, since it was in the center of only a twenty foot span from one side of the small ditch to the other, it was obvious that the central post was designed to support a heavy weight, no doubt an artillery piece. The fire burned area represented a seventeenth century hearth area around the central post, apparently used by those who were charged with the operation of the artillery piece on the platform above. The archeological evidence, therefore, clearly indicated enough architectural data to allow for a good idea as to the appearance of this feature. From the large dry redoubt ditch almost no artifacts were recovered from the top two-thirds of the ditch. The bottom layer, however, was rich with large pintles, hinges, fragments of Delft tiles, lead fragments, and many handfals of casting sprues for lead swan shot, and musket balls. Such a quantity of discarded sprues would seem to be a waste of good lead, but apparently there was enough on hand so that the sprues could be discarded rather than being melted again to make more shot. One fragment of lead sheeting had the word "Gray" written in script, and likely was once owned by Thomas Gray who was a Charles Towne leader from 1670 until 1673. He was a military leader, as well as a member of parliament and

the council. He lead expeditions against the Kussoes, Westoes and Stonos. He lead civil disturbances in 1673, and his property was seized by the government, and he fled the Province.²⁵ It appears then, that the lead sheet with the name "Gray" would date between 1670 and 1673. The transfer printed Delft tiles, wine bottle fragments, and other artifacts dating to almost 1800, found here also, reveal how long the redoubt ditch stood open.

From the archeological evidence, therefore, it would appear that what is represented here is a redoubt constructed of vertical timbers placed in a ditch, forming a chamber for the storage of equipment and supplies for the artillery piece above it. If a normal weight were placed over the top of this chamber there would be no necessity for a central supporting post for the twenty foot span from one wall to the other. However, if an artillery piece were placed overhead, a central supporting post would be necessary. The arc of the fan shape of the ditches of the redoubt would indicate the arc of fire to be covered by the artillery piece. The fact that only twenty feet of space was available over the chamber in which to operate the artillery clearly indicates that only one piece was involved. The fact that the redoubt face was shaped in an arc would indicate that the artillery piece would not have been mounted on a ship carriage, which would have allowed only a limited range of fire, but would have been mounted on a trail type field carriage. This fact is apparent even though there is reference only to ship carriages being brought into Charles Towne. The evidence of many fires around the central supporting post for the chamber, plus the presence of casting sprues for making swan shot, would indicate that the men who were manning the artillery piece were melting the lead and casting shot inside the chamber. The fires were likely kept fairly small to prevent setting fire to the central post. Access to this chamber beneath the artillery piece was likely by a ladder through a trap door in the floor of the gun chamber. The outside of the chamber wall was protected by earth thrown from the fan-shaped fortification ditch surrounding the chamber.

The artillery piece which sat in this redoubt was likely a demi-culverin which was the longest range piece at Charles Towne. In 1669, when the Carolina expedition was being equipped, the following was loaded on the vessels:

...four iron demi-culverin and eight sacres, with ship carriages, ladles, sponges and linstocks & 12 rounds of shot for each....²⁶

The barrel of a demi-culverin weighed 3600 pounds, and was the reason for the necessity for the central supporting post in the redoubt.²⁷ The demi-culverin on a trail carriage would have allowed a sweeping fire of the entire area of deep water in front of Albemarle Point. The ship carriage mounted guns behind the parapet of the main fort

ditch would have provided protection only if the enemy came within the line of fire. A redoubt with a demi-culverin on trail carriage in front of the main fort would have provided the flexibility of fire not possible with the fixed position ship carriage mounted guns.

This arrangement of the fort was not unique in that it employed methods for defense well known at the time; however, the utilization of only a reentered angle with no bastions, and a separate redoubt providing flexibility of fire in front of the fort, is a unique adaptation of fortification principles to fit the particular need at Charles Towne, and an exact parallel is unknown. The "V" shaped reentered angle at the entrance would have provided an excellent means of protecting the entrance into the fort with crossfire. A similar entrance through the reentered "V" angle is seen in details from Clampe's Plan of the Neward seige during the First Civil War in England in 1646.²⁸

The consultant for the interpretive reconstruction of the redoubt was Harold L. Peterson of the National Park Service. The original interpretive drawing was based primarily on archeological data and revealed a central chamber with earth from the surrounding ditch thrown up against the vertical timber walls of the chamber for protection. After the archeologist had executed this drawing, Harold Peterson found a drawing made by Thaddius Kosciuszko, in the latter part of the eighteenth century, showing Redoubt #4 at West Point.²⁹ This drawing of two sections of the West Point Redoubt, though one hundred years later than the Charles Towne Redoubt, is the closest known parallel to it. The West Point Redoubt was apparently much larger than the one at Charles Towne, and instead of a single supporting post for the gun platform, there were several. The outer wall of the redoubt was constructed of timbers placed horizontal to the ground in a cribbed manner, and the area between the outer cribbed wall and the inner chamber wall was filled with earth. The height of this cribbed wall was at least six feet above the level of the gun deck to provide protection for the gun and crew. The guns were fired through openings in this cribbed, earth-filled wall. Since one hundred years had elapsed between the construction of the Charles Towne Redoubt and the West Point Redoubt drawing, it is unwise to draw identical parallels between the two features. However, horizontal cribbing would not reveal archeological evidence of its existence and therefore, the interpretation of such a feature must come from drawings rather than from archeology. Since we know that the chamber beneath the gun at Charles Towne was likely at least six feet high, and, since the protective wall for gun and gun crew above the chamber would have had to have been about six feet high to effectively offer protection, we can safely say that there had to be a protective wall at least twelve feet high around the redoubt chamber. An earthen wall this high on only a ten foot base could most effectively be kept in place by means of a cribbed log wall such as shown in the West Point Redoubt. It is thought, therefore, that the Charles Towne Redoubt very likely had a cribbed log wall similar to that shown for the West Point Redoubt.

Archeological evidence supporting this is the fact that from the redoubt ditch artifacts dating throughout the eighteenth century were recovered, indicating that the ditch was not filled until around 1800, whereas artifacts from the main fortification ditch do not date after the seventeenth century. One of the prime reasons why the smaller redoubt ditch should stay open one hundred years longer than the deeper and broader main fortification ditch, would be the presence of a cribbed log wall supporting the sand thrown from the redoubt ditch. The main ditch, not being cribbed, would erode with every rain and stroke of the farmer's plow, whereas the cribbed redoubt would stand virtually untouched until rot finally released the burden of sand held within the cribbed logs, allowing it to wash into the redoubt ditch, finally filling it about 1800. Thus we have archeological support for the cribbed log redoubt interpretation as well as the Kosciuszko drawing of the Redoubt at West Point.

With this information from archeology, and the redoubt drawing, the question was raised as to whether reconstructing the Charles Towne Redoubt from the interpreted archeological drawing was justified. The project was estimated to cost between \$35,000.00 and \$40,000.00 to rebuild the redoubt, which would have been possible using broad ax and adz craftsmen to hew and fit the logs. If the West Point drawing had not been one hundred years later than the known date of the Charles Towne Redoubt, no hesitation at rebuilding the redoubt would have been encountered. However, although it was very likely that the conjectural drawing resulting from the joining of the archeological and documentary data bore a close similarity to the appearance of the original redoubt, it is extremely unwise to undertake full-scale reconstructions with such a time gap between the archeological and documentary evidence. For this reason a rebuilding of the redoubt was not recommended. Instead, the earth parapet accompanying the main fort ditch, and a mound of dirt around the redoubt circle, was recommended as a means of interpretive explanation to the visiting public. This interpretive explanation has been carried out and the entire fort sodded with grass. Since the Spanish spy Camunas mentioned in 1672 that there were twelve artillery pieces facing the deep water and the port by which they entered, eleven openings were cut into the parapet, with the twelfth gun being on the redoubt. The position of these openings is not known, and were placed at what appeared to be appropriate and practical locations along the parapet. This may appear to some to be carrying the interpretive explanation too far, but, as Harold Peterson has pointed out, to avoid placing such openings for artillery pieces because the exact position of the original is not known, would be a greater error than placing such openings at assumed locations along the parapet.

It is assumed that there would have been a bridge across the dry ditch at the entrance "Y", and some means for the gun crew to get from the redoubt to the inside of the fort. Therefore, visitor access bridges should be constructed at these two points to allow access to the

top of the redoubt mound and to the area in front of the fort.

There may have been palisades accompanying the fort parapet, or perhaps a fraise fastened into the rampart of the fort and redoubt, but no archeological or historical data definitely associates such a feature to the Charles Towne fort on the point. The references to palisades were apparently to the palisade found to have extended around the land side of the fortified area. The West Point Redoubt #4 drawing shows that redoubt furnished with a fraise, but it is unwise to apply this feature to the Charles Towne interpretation without further supporting evidence.

In summary of the fortification evidence for the Charles Towne settlement of 1670-74, we know that a broad ditch with a redoubt in front was designed to protect against possible Spanish attack by water, and a small ditch with an accompanying breastworks and palisade was located along the land side of the tip of Albemarle Point, providing a fortified area from which the town could be protected. It is interesting to note that Lord Ashley in discussing the fortification necessary for a port town said:

A Pallisado round the Towne with a small Ditch is a sufficient Fortification against the Indians.³⁰

The palisade and small ditch found through archeology fits this description well, and the mound accompanying the "small Ditch" along the mainland side at Charles Towne was seen and described by Camunas in 1672 as a "low embrasure."³¹

The broad trench of the main fort facing the water entrance to Albemarle Point brings to mind the reference which stated that the town:

...may easily be strongly fortified wth a broad trench, it contains about 10 acres of Land.³²

The land lying between the broad trench at the redoubt and the small ditch along the land side of the area is found to contain slightly over ten acres. The archeology at Charles Towne has clearly revealed, therefore, the location of the fortification constructed by the first colonists around a ten acre tract of land for the protection of the Charles Towne settlement. Positive evidence for "some lodgings" inside and outside of this fortified area which "were built at first when they began to settle for fear of the Indians,"³³ was not found archeologically. Evidence for the location of the major town itself, outside this fortified area to the northwest, will have to await further archeological exploration in the years to come.

FOOTNOTES

¹Langdon Cheves (ed.), Collections of the South Carolina Historical Society; "The Shaftesbury Papers and Other Records Relating to Carolina and the First Settlement on Ashley River Prior to the Year 1676." (The South Carolina Historical Society. Charleston: 1897, V, Preface 3-4, 167-169. Hereinafter cited as S.P.).

²S.P., V, 169, 174, 196-197, 210.

³S.P., V, 332, 339, 254, frontispiece.

⁴S.P., V, 93.

⁵S.P., V, 120, 179, 283.

⁶Jose Miguel Gallardo, "The Spaniards and the English Settlement in Charles Towne," The South Carolina Historical and Genealogical Magazine, XXXVII, No. 2, (April, 1936), 57. Hereinafter cited as S.C.H.G.M.).

⁷S.P., V, 283.

⁸Library of Congress document A.G.I. 61-1-18, Mexico, enclosure in a letter of Cendoya dated December 15, 1672, translated by Dr. St. Julien Childs. Hereinafter cited as A.G.I. 61-1-18.

⁹S.C.H.G.M., XXXVII, No. 3, 95-98.

¹⁰Harold L. Peterson, Round Shot and Rammers. Harrisburg, Pennsylvania: 1969, p. 25; Albert Manucy, Artillery Through the Ages. United States Government Printing Office, Washington: 1949, p. 9.

¹¹South Carolina Tricentennial Commission Research Material. Document 58-1-26. Letter dated July 26, 1671, from Pacheco to Cendoya, taken from photostats of the Stetson Collection and the North Carolina Archives. Hereinafter referred to as 58-1-26.

¹²S.C.H.G.M., XXXVII, No. 3, 96; S.P., V, 462.

¹³S.P., V, 378-9; S.C.H.G.M., XVI, No. 1, January, 1915, 5.

¹⁴Alexander S. Salley, Jr. (ed.), Narratives of Early Carolina 1650-1708. New York: 1959, p. 186.

¹⁵A.S. Salley, Jr. Records in the Public Record Office Relating to South Carolina 1685-1690. (The Historical Commission of South Carolina), Atlanta: 1929.

¹⁶S.C.H.G.M., XVI, No. 1. January, 1915, 6-7.

¹⁷Louis de Saint Pierre, The Art of Planting and Cultivating the Vine; as also, of Making, Fining, and Preserving Wines, etc. According to the Most Approved Methods of the Most Celebrated Wine-Countries in France, Compiled for the use of such as intend to prosecute that beneficial and National Branch of Commerce and Agriculture in America, and Particularly for that of the Colony at New Bourdeaux. Printed by J. Wilkie, London: 1772, xxvii. Hereinafter cited as The Art of Planting. Copy of The Charleston Library Society, F66 RG. Sa 2.

¹⁸A.G.I. 61-1-18.

¹⁹Edwin A. Barber, "Antiquity of the Tobacco-pipe in Europe," The American Antiquarian. Vol. II, 1880, 117.

²⁰Edwin A. Barber, "Early European Pipes Found in the United States." The American Antiquarian. Vol. IV, 1882, No. 3, 198.

²¹Thomas Sheppard, Early Hull Tobacco Pipes and Their Makers. September, 1912, 3-39. (Mr. Sheppard also states that the Hull "EB" pipe had a milled line below the rim, as does the Charles Towne specimen. He says it is a later 17th century pipe.).

²²John E. Pritchard, "Tobacco Pipes of Bristol of the XVII Century and Their Makers," Transactions of the Bristol and Gloucestershire Archaeological Society. Kendal: 1923, 165-191.

[The above notes, 18-22, are from a collection of papers owned by Elias Bull, and presented to the South Carolina Archaeological Society.]

²³Ivor Noël Hume, personal communication.

²⁴A. S. Salley (ed.), Records of the Secretary of the Province and the Register of the Province of South Carolina 1671-1675. (Historical Commission of South Carolina, Columbia: 1944), 27. Hereinafter cited as Records of the Secretary.

²⁵S.P., V, 222, 342.

²⁶S.P., V, 93.

²⁷ Harold L. Peterson, Round Shot and Rammers. Harrisburg, Pennsylvania: 1969, 14.

²⁸ Newark on Trent, The Civil War Siegeworks. (Royal Commission on Historical Monuments [England], Her Majesty's Stationery Office, London: 1964, Plate 9.).

²⁹ "McDougall Papers", Redoubt #4, West Point, drawn by Thaddius Kosciuszko. Courtesy of The New York Historical Society, New York City. Negative No. 49593.

³⁰ S.P., V, 343.

³¹ A.G.I., 61-1-18.

³² S.P., V, 309.

³³ A.G.I., 61-1-18.

Figure 1

A Map of the Exploratory Excavations at the Site of the 1670-1680 Settlement of Charles Towne on Albemarle Point at Kyawaw on Ashley River in South Carolina - November 12 to December 20, 1968.

In 1969, a nine month excavation was carried out on this site resulting in the discovery of a redoubt fortification ditch. A detailed drawing of this feature is seen in Figure 2.

(See Map in Jacket Pocket)

Figure 2

The Archeological Plan and Profile of the Redoubt Area at the 1670 Fort at Charles Towne (CH1-1) with the Interpretive Architectural Section Including a Tracing of a Drawing by Thaddius Kosciuszko of the Eighteenth Century Redoubt #4 at West Point, New York.

(See Map in Jacket Pocket)



Figure 3



Figure 4

Figure 3

The Junction of the North Fortification Ditch in 1670 (to the left) Excavated and Left Open with its Accompanying Embankment, with the Unexcavated West Fortification Ditch (to the right) Just Prior to Excavation. Machines were Used to Strip the Disturbed Plowed Zone from the Excavated Areas to Reveal the Fortification Ditches and Related Features. The Bottom of the Palisade Ditch Accompanying this Fortification Ditch was Found in Several Places Along the North Ditch, Directly Beneath the Center of the Rebuilt Embankment.

Figure 4

The Junction of the North and West Fortification Ditches After Accompanying Embankments Were Rebuilt and Stabilized with Sod. An Irrigation System Supplied Water for the Sod Until it was Firmly Established.

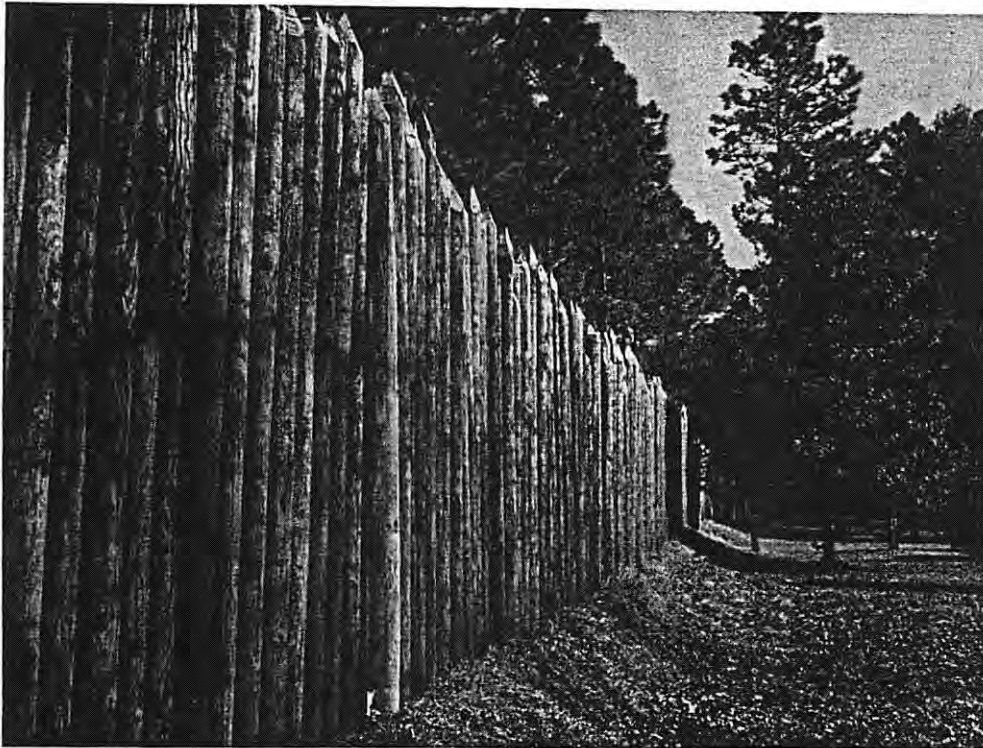


Figure 5



Figure 6

Figure 5

The Palisade Wall Replaced in the Original Position Along the North Fortification Ditch at Charles Towne.

Figure 6

Inside View of the Main Fortification at Charles Towne Showing the Rebuilt Parapet with Firing Step Stabilized With Rolls of Sod. Explanatory Exhibits Such as This Clearly Reveal the Location of Archeologically Determined Fortification Features.

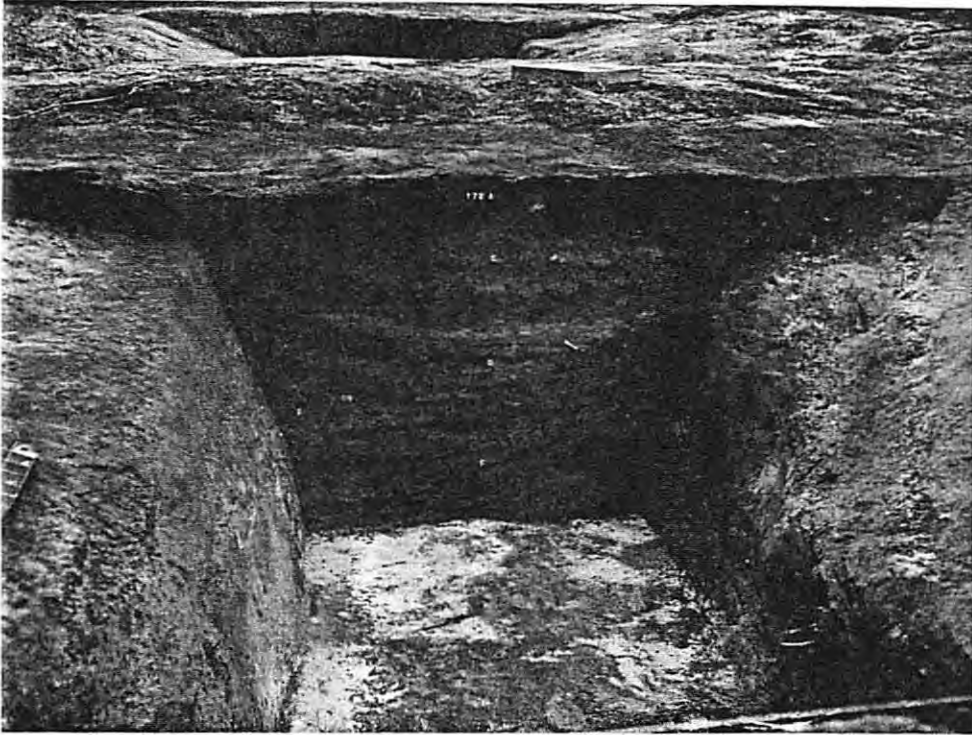


Figure 7



Figure 8

Figure 7

Profile of the Main Fortification Ditch at Provenience Area 172. Artifacts from the Ditch Reveal that it was Probably Filled by Around 1700.

Figure 8

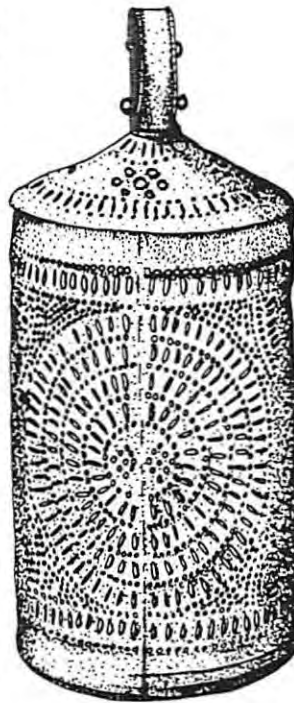
View of the Main Fortification Ditch with Circular Redoubt Ditch in Front, with the Deep Water Channel of Old Town Creek in the Background. The Size of the Accompanying Parapet Embankment was Determined by the Volume of the Soil Found to Have Been in the Original Fortification Ditch. Historic Site Development Through Such Explanatory Exhibits Are an Excellent Means of Bridging the Gap Between the Archeological Fortification Ditch as Seen by the Archeologist and the Visitor to the Historic Site. With Accompanying On-Site Map Markers and Visitor-Center Interpretation, Fortification Features Such as This Can be Interpreted in a Meaningful Manner.



A



B



C



D



E

Figure 9

Figure 9

- A- A Very Fragile Pierced Iron Lantern Cone Found in the Bottom of the Redoubt Ditch (38CH1-207C-38) in Association with Artifacts Dating No Later Than 1800. The Consistency of the Metal was That of Flakey Pie Crust, and Preservation Procedures were Undertaken Immediately Upon Removal from the Ground.
- B- The Pierced Iron Lantern Cone After Completion of the Preservation Process the Following Day, Ready for the Study Collection or Museum Exhibit.
- C- The Type of Pierced Iron Lantern Represented by the Cone, Which is Typical of Eighteenth and Nineteenth Century Lanterns. (From Illustration 115 in Know Your Heirlooms by Thomas H. Ormsbee. The McBride Company, New York: 1957.) Ormsbee Dates the Type ca. 1750-1825.
- D- "Diogenes in Search of an Honest Man" Using a Pierced Iron Lantern Similar to the Charles Towne Example. This Painting by Salvator Rosa (who died in 1673), Reveals That This Type Lantern was Used at Least This Early. Painting Reproduction from Pictorial History of Philosophy by Dagobert D. Runes. Philosophical Library, Inc., New York: 1959, p. 99.
- E- British Infantryman Drummer, Scots Fusilier Guards, Making "Night Rounds", About 1832, Using a Pierced Iron Lantern. Painting by A. J. Dubois Drahonet from Military Drawings and Paintings in the Collection of Her Majesty the Queen by A. E. Haswell Miller and N. P. Dawney, Vol. 1, Plate #397, (Cat. No. 2154). The Phaidon Press. London: 1966.

THE GUNFLINTS OF SEPT-ILES AND MINGAN

T. M. Hamilton

Introduction

In 1965 Father Rene Levesque of the Centre d'Etudes Nordiques, Université Laval, Quebec, Canada, asked me if I would be interested in making a study of the gunflints recovered in their excavations at Sept-Iles and Mingan; two early trading post sites lying near the mouth of the St. Lawrence in the Province of Quebec. The gunflints were received in January of 1966, and the study completed by June of that year. The original plans were that it was to be published first in French, but unforeseen difficulties have prevented this, so Father Levesque and I have agreed that it would be best to proceed with the English publication without further delay.

Since this paper was written, John Witthoft's "A History of Gunflints" has appeared in the 1966 issue of the PENNSYLVANIA ARCHAEOLOGIST (printed in January of 1968), but I have decided that in the interest of maintaining an historical perspective on the gradual accumulation of basic information in the modern study of gunflints, that it would be best to publish this paper exactly as it was written in 1966. Actually, I know of no place where our more recent studies would dictate any important changes in my conclusions as here set forth.

Father Levesque's paper, which is a commentary upon my findings, immediately follows and should be considered a part of this study.

The study of gunflints is in its infancy. Very little has as yet been written about them, and most of that for popular consumption is misleading. A compilation of the more pertinent papers written before 1960 will be found in INDIAN TRADE GUNS, (Hamilton, 1960). For the French reader there is also the HISTOIRE DE LA PIERRE A FUSIL by Jean Emy and Bernard de Tinguy, published by the Musée de la Pierre à Fusil, Meusnes (Loir et Cher), France., 1964. Since I am ignorant of French I am in no position either to criticize this publication or recommend it, but, judging from the illustrations, I would expect it to be quite reliable. Plate three, for instance, shows how the English made their flints and explains why this produced the small demi-cones of percussion on the sides of the individual gunflints.

A paper by John Witthoft, of the Pennsylvania State Museum, also entitled "A History of Gunflints", is now in the hands of the editor of the PENNSYLVANIA ARCHAEOLOGIST and will be published sometime in 1966. Mr. Witthoft has furnished me with a manuscript of this important paper and I will refer to it from time to time, but it must be born in mind that certain changes may be made in the final draft before publication. I am mentioning this if any discrepancies should appear.

To date, Mr. Witthoft has done most of the basic research on gunflints, but Carlyle S. Smith of the University of Kansas has also made some substantial contributions. Aside from those two men I am unaware of

any archaeologists who are concerning themselves with the problem. Since Mr. Witthoft's manuscript presents the most recent developments, I shall summarize it briefly.

According to Witthoft there are three basic types of gunflints:

First, the Nordic, used from about 1630 to 1675. It tended to be square in shape, and was chipped on both faces, and at first glance appears to be of Indian workmanship. However, the flint is of high quality and, once one has become acquainted with European flints, is not too difficult to recognize. It was made from flint mined in Denmark, hence its name.

The second type was the Dutch, used from about 1650 to 1750. This is the gunflint which I have called the "gunspall" in previous papers. (Hamilton, 1960: 73-79 and 1964: 52-57), and Maxwell and Binford have referred to as "spall gunflints" (1960: 100). Witthoft, in both correspondence and manuscript, has used the terms "wedge-shaped spall" and "Clactonian" but seems to now prefer the term Dutch, "with the understanding that Holland at that time was much larger than now and included Belgium" (Letter to TMH dated 3/3/66). For reasons which will become obvious in the next few paragraphs, I feel that the term "Dutch" is a very happy selection, and shall use it with certain modifications.

The third type, made from blades, struck from a polyhedral flint core, was introduced by the French around the year 1680 and used until the close of the flintlock era, or about 1850. There are two basic types of these gunflints, the French and the English.

The French gunflint was usually made of a beeswax chalcedony, with a rounded heel and much secondary chipping around both the sides and the back as shown in Figure 3, A to G. It is usually found on 18th Century sites. Its period of greatest popularity was from the early 1700's to about 1780.

The English gunflint was usually made from a dark flint rather than a chalcedony and is doubled edged. There are, of course, exceptions. There are even occasions where we find an English gunflint made from a French blade, as in the case of Figure 1, G. As Witthoft points out in his manuscript, the gunflint of English manufacture is distinguished from the French by a demi-cone of percussion found on each side as shown. The best way to detect this is to run the finger along the side of the flint. Since Witthoft describes the difference in manufacture in detail, I will not go into that here. The English gunflints begin to appear on American sites in limited quantities around 1775 and gradually came to dominate the trade. My own investigations on the upper Missouri to the Canadian border substantiates this for we find there that the English gunflint appears to have been the most popular by far during the first half of the 19th Century when the American and English traders were exploiting the country. French flints, when found, usually represent 18th Century commerce in that area.

I should point out that the date brackets given here for the various

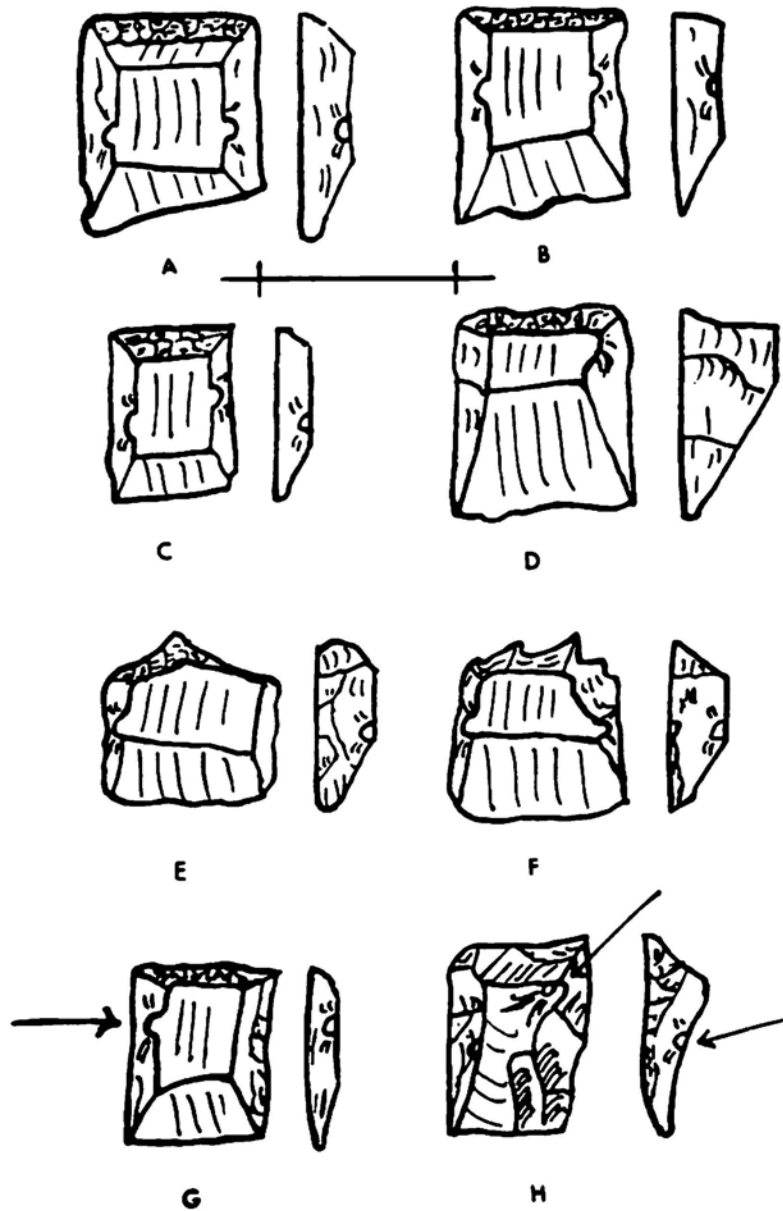


FIGURE 1

ENGLISH STYLE GUNFLINTS, MOSTLY FROM SEPT-ILES

FIGURE 1

ENGLISH STYLE GUNFLINTS, MOSTLY FROM SEPT-ILES

All are single edged.

A, B, and C are of excellent quality, both in material and workmanship. The flint is actually a brown chalcedony.

D, E, and F represent the majority of the flints, which are made from regular black flint and are of indifferent workmanship. On many of these the back edge has been broken away in a very unprofessional-like manner; probably in the field by the user.

G is an English gunflint made on a French beeswax blade. Note the demi-cone of percussion on the left side of the top face.

H is an unusual form of the Dutch gunflint. The bulb of percussion, left when the Clactonian flake was knocked from the nodule, is shown by the arrow on the left, and the demi-cone of percussion, left when the side flake to the left of the drawing was removed, is shown by the arrow at the right showing the side view.

The general style of this gunflint looks English to me, as well as the technique used in removing the side flake. The flint is a black chalcedony. I have never seen a gunflint like this before. It belongs in Subtype C.

All flints, except G, from Sept-Iles. G is from Mingan.

Catalogue numbers, A to H: 187A,a; 650E; 220A,b; 266A; 38a; 185A,g; 194A,a; and 1923A.

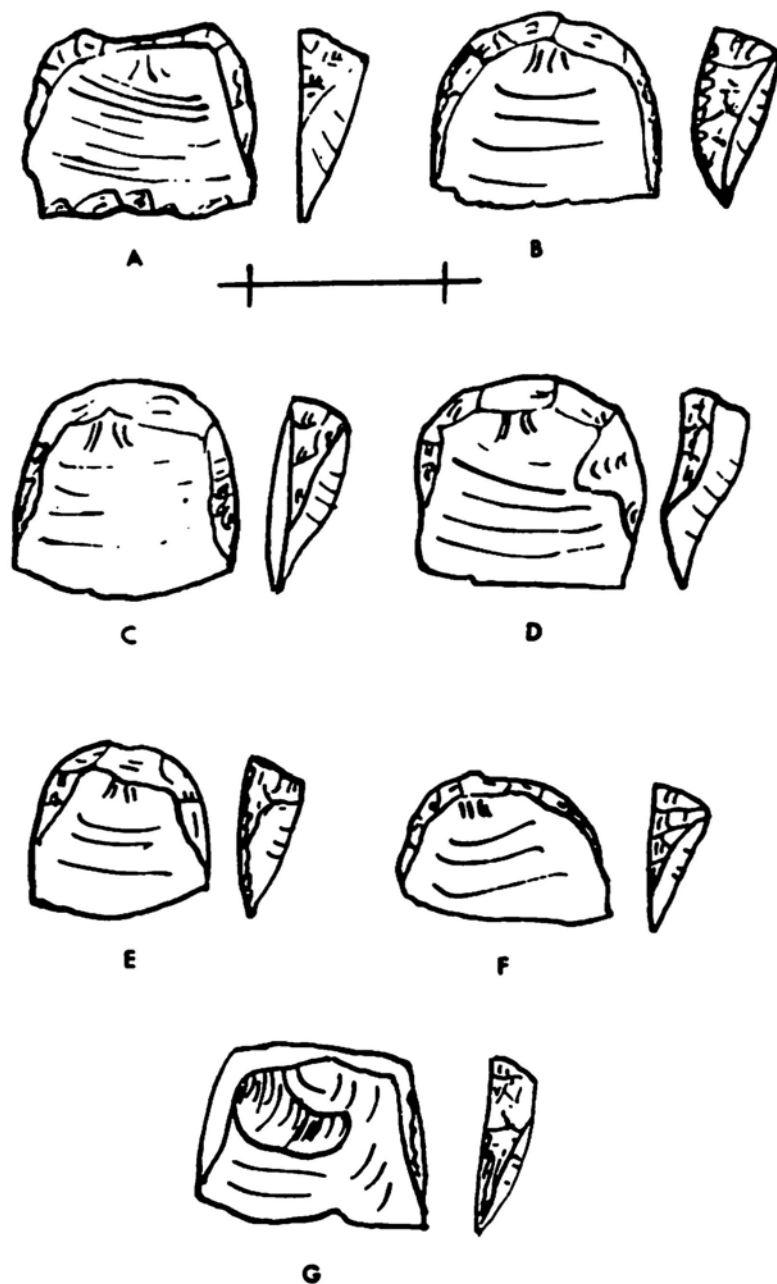


FIGURE 2
DUTCH GUNFLINTS FROM MINGAN

FIGURE 2

DUTCH GUNFLINTS FROM MINGAN

All of these, except G, are presumably from the Riss outwash. Formerly, I would have classified A as belonging to Subtype A because it is a chert; a streaked, brownish chert with an almost granite-like texture.

B to F would have fallen into my Subtype B, but are now found to correspond to flints identified as having come from the Riss outwash. They are all of a fine textured greenish-black flint.

G definitely belongs to Subtype C, for it is made from a French beeswax chalcedony, a known mined flint, and should date from 1680 to 1750, approximately.

The catalogue numbers from A to G: 5480; 5261; ??3F; 488?; no number; 5762 and 3641.

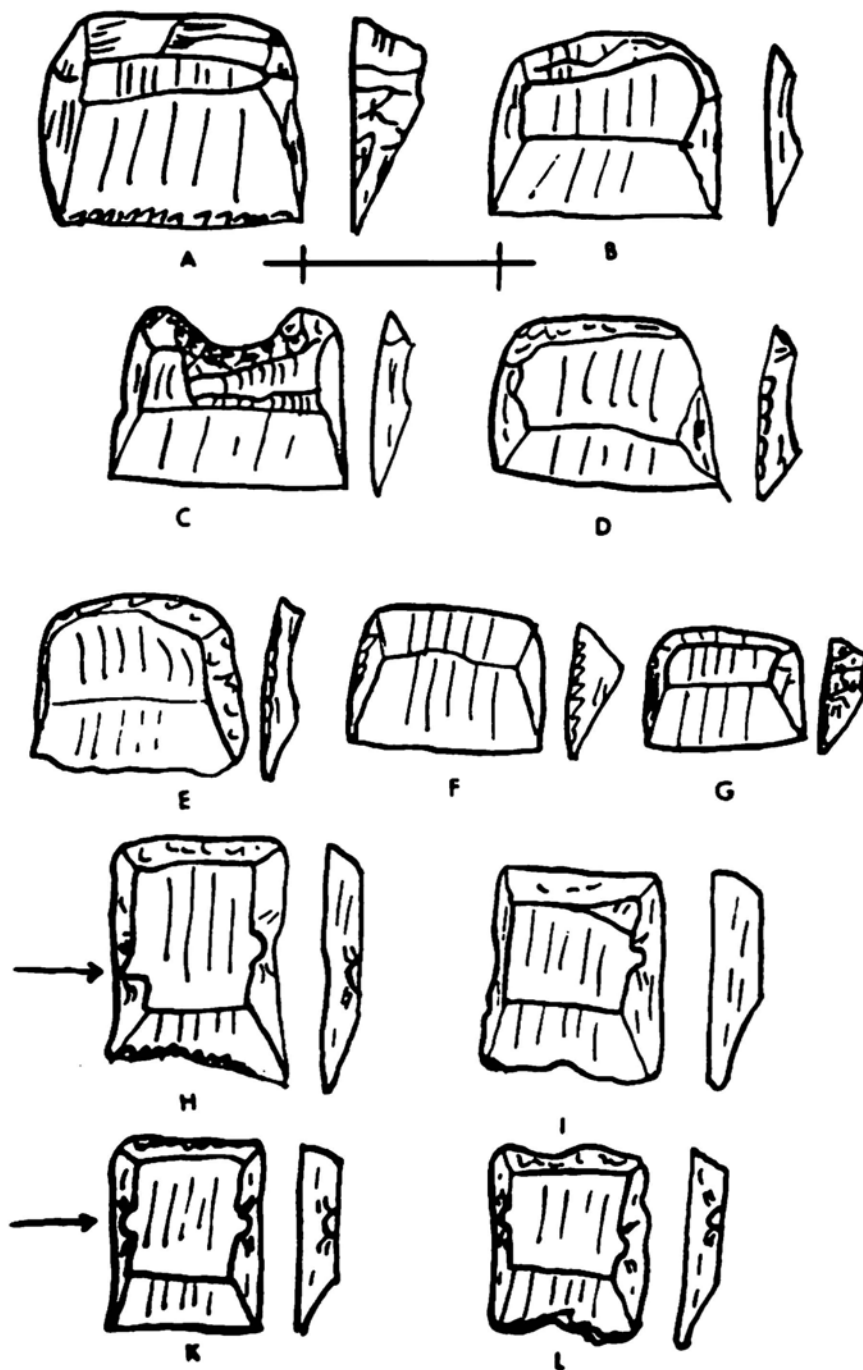


FIGURE 3
GUNFLINTS FROM MINGAN

FIGURE 3

GUNFLINTS FROM MINGAN

A to G: Conventional French gunflints of beeswax chalc-dony, all in original or near original condition, except for C whose back has been chipped away from use against a fire-steel.

H to L: Early style English gunflints (around 1800) with single edges. Note demi-cones of percussion left on sides (arrow) due to English technique of breaking flint blades into individual gunflints. When found on a flat face, such as these, this is an infallible sign of English manufacture, even when the blade itself is of French origin. All of these have been used except for K.

Catalogue numbers from B to L. A has no number. 3534; 5766; 1403; 3296; 1198; 3317; 5485; 5254; 2129; and 1130.

types of gunflints may vary somewhat from those given by Witthoft and when that occurs represent my own modification. For instance, I tend to favor an earlier date for the initial introduction of the French gunflint into America. In a previous paper of mine (Hamilton, 1964: 55) I suggested 1680 as the probable approximate time for its initial appearance. This was based partly upon an analysis of the flints from accurately dated Seneca sites in the Charles F. Wray collection, and I have seen nothing in the meantime which seriously challenges this conclusion. Witthoft in his manuscript concedes that "A few French flints may have been made before 1675", but believes that "They were not an ordinary article of commerce until later than 1740."

I hope that Mr. Witthoft is eventually proven right, for a later date for the introduction of French flints would simplify the use of gunflints for dating purposes. However, as I view the evidence so far available, it seems that there was a more or less gradual change from one type of gunflint to another rather than clean, clear-cut, abrupt changes, and the proportion of one type to a succeeding type in any given collection must be nicely balanced to furnish supporting evidence in dating.

For instance, the Wray collecting breaks down as follows:

Site	Date	Native	Nordic	Dutch	French or English	Unclass- <u>ified</u>	Total
Cummings	1625-50	17					17
Steele	"	51		2			53
Power- house	"	223	1	4		1	229
Marsh	1650-75	74	5	2		1	82
Dann	"	253	8	16		5	282
Boughton Hill	1675-1687	13	1	11		2	27
Rochester Junction	"	37	8	11			56
Kirkwood	"	4		2			6
Beal	"	14	1	18	6	2	41
Avon	1750-79	1		10	4		15
Tonowanda	1779-1820				4		4

From the above it is obvious that Mr. Witthoft has developed a thesis of great promise which deserves our most careful attention. His work on gunflints has been brilliant and, so far as I can see, with a few changes in

minor details as our experience broadens, it can become a valuable tool in dating sites from about 1625 to around 1850.

However, as with all dating tools, it must be used with judgment born of experience. The gunflint problem is complex and filled with surprises if not outright exceptions. An examination a few years ago of gunflints from an Indian village near the mouth of the Rock River, thoroughly documented and known to have been occupied only during the first quarter of the 19th century, had a surprisingly large number of Dutch flints in it. Further comment will be reserved until I see the final report on the excavation and have an opportunity to thoroughly examine the gunflints again in the light of today's knowledge. A recent visit to Fort Frederica, Saint Simons Island, Georgia, occupied by the British from 1736 to about 1750, produced some evidence to support the interpretation (Manucy, 1962: 42) that gunflints were actually made there. A final decision will have to await recovery of the archaeological evidence from storage, but I have seen enough to make me suspect that the product was "Dutch" gunflints made from English flint brought over as ballast. (Speaking of complications!) If it is proven that gunflints were actually made there, it will be the first instance yet known where professional white flint knappers produced gunflints on the North American continent. All the evidence to date has pointed to European sources for the finished gunflints throughout the flintlock era.

The only real criticism I can make of Witthoft's thesis is his contention that all Dutch gunflints were made from material from the Riss outwash. In the same paper where I advanced the 1680 date for the initial introduction of the French gunflint I also suggested that the Dutch flints be subdivided into three categories; Subtype A, representing those made of chert; Subtype B, those made from flint of unknown origin; and Subtype C, those made from flint matching known quarries or the material used in conventional French or English gunflints. With the developments now brought forth by Mr. Witthoft, I suspect that Subtypes A and B are mostly from the Riss outwash and can be assigned to limbo. However, in my opinion, Subtype C is still very much with us.

The matching of flints is far from a scientific process. In spite of many attempts to reduce it to a purely objective basis, it still remains a subjective activity masquerading under the impressive name of "megascopic analysis". Therefore, it reduces finally to being little more than one man's opinion against another's. You shall see in the analysis of the gunflints in the first lot from Sept-Iles that I find three Dutch flints belonging to the Subtype C Category.

THE GUN FLINTS FROM SEPT-ILES

Initial comments:

I know very little about Sept-Iles other than some general information given me by Father Levesque when we initially discussed my examining the gunflints recovered from that site. Since my primary interest is in testing,

developing and refining Witthoft's basic theories on the use of gunflints for dating purposes, I have deliberately refrained from rereading Father Levesque's letters, for fear that my conclusions may be in some way influenced. I do remember that the site was originally occupied sometime during the middle of the 17th Century by Joliet and that it, of course, came into English possession following the fall of Quebec one hundred years later.

I know nothing whatever about Mingan. If Father Levesque mentioned any details concerning it in his letters, it has completely escaped me.

I notice that Witthoft in his writings often speaks of gunflints by size; musket, carbine, pistol, etc. Lewis (1960: 160) even gives the maximum and minimum allowable dimensions as shown in the U. S. Army Ordnance Manual for 1849 for musket, rifle, and pistol flints, but they simply do not seem to conform in any way with the gunflints I examine from archaeological sources.

When I first examined the flints from Sept-Iles it was immediately obvious that the English gunflints, particularly, fell naturally into three sizes. It was tempting to call the larger size "musket", the second size "trade gun" and the smallest "pistol", but since I could not justify those terms from any documentary sources available to me I decided to simply list them as "Large" (over 7/8 inches wide), "Intermediate" (5/8 to 7/8 inches wide) and "Small" (under 5/8 inches wide). I have not listed any other dimensions since they are more or less meaningless and would result in unnecessary complications. The lengths of archaeological gunflints are particularly meaningless since very few of them are recovered in original condition, and, judging from the wide variations in thickness of the gunflints before me, I find it hard to believe that the customers were very particular about that.

According to the U. S. Army Ordnance Manual of 1849, the nomenclature used with gunflints was as follows: The edge or bevel, the back, the sides, the face, and the bed or lower face.

The material from Sept-Iles was received in three separate lots. Though the first lot is unmarked, it presumably represents the flints excavated during 1962-63, for the other two are marked "1963-64" and "1964-65" respectively. Therefore, I shall designate these lots as 1, 2, and 3 respectively and examine them individually since I presume that there is some interrelationship here of which I am unaware. Later they will be considered collectively in the summary.

Lot I, (1962-63?)

- 36 Dutch gunflints from the Riss outwash (Subtypes A and B).
- 3 Dutch gunflints of French beeswax chalcedony, (Subtype C).
- 7 French gunflints of beeswax chalcedony.
- 3 French gunflints unidentifiable because of burning.
- 135 English gunflints.

Comments:

Subtype C Dutch gunflints #37a and #108A match the beeswax chalcedony used in French gunflints #195-A-a and #6. Subtype C Dutch gunflint #35e matches French gunflint #198-4.

The proportion of Dutch gunflints to French is 39 to 10, or 4 to 1. Three of the Dutch gunflints are Subtype C which would give them a post 1680 date and all of the French gunflints should be at least that late. Because there are relatively few French flints to Dutch I would say that the lot represents an occupation from about 1650 to the early 1700's, say 1720 or 1730. It could be either English occupation or French, so far as the gunflints are concerned, but since I know it was in French territory at the time it was obviously French.

The proportion of English to Dutch-French is 135 to 49. The presence of French gunflints (Hamilton, 1960: 75) does not necessarily indicate French traders or occupation, but the presence of English flints almost invariably means English activity. Therefore, I would say that the English flints represent an entirely different and later occupation than the Dutch-French flints, most probably after the year 1800. After viewing these flints, Witthoft (undated note) commented that the English gunflints could very well represent a military camp of the war of 1812.

To me these English flints, as a group, are particularly interesting because all of them, without a single exception, are single edged. Furthermore, most of them are crudely made. They are not at all like the neat, almost machine-made, examples of the English gunflint ordinarily found on sites of the upper Missouri.

Many of these English flints were obviously made one-edged originally, but many more have had their rear edge knocked or dubbed off in a very unprofessional-like manner, which indicates to me that the men at this outpost, for some reason or another, had an aversion to double edged gunflints and proceeded to make corrections forthwith.

There is one neatly made English gunflint, #94A, on a French blade (Figure 1, G) measuring 11/16 inches in width. Whether this has any significance or not I do not know, but is worth mentioning. (See Witthoft, 1966: 32.)

The English gunflints consist of 9 large, 106 intermediate, and 11 small.

Lot 2, 1963-64

- 8 Dutch gunflints from the Riss outwash.
- 1 Dutch gunflint, Subtype C, #1923A.
- 1 French beeswax gunflint, 1½ inches wide.
- 6 English gunflints, large.
- 13 English gunflints, intermediate.
- 4 English gunflints, small.

Comments:

The proportion of Dutch to French is 9 to 1. The proportion of English to Dutch-French is 23 to 10.

The Dutch gunflint, Subtype C, #1923A is certainly not typical. It was originally detached from the nodule as an individual spall, for the face and bevel are one continuous slope with a distinct bulb of percussion still present. However, it has been dressed on the sides and back in the English manner. (See Figure 1, H.) I have never seen a gunflint of this sort before.

The English gunflints are all single edged.

So far as occupational dates or nationalities are concerned, I see nothing significantly different from Lot 1.

Lot 3, 1964-65

- 36 Dutch gunflints from Riss outwash.
- 11 Dutch gunflint fragments from Riss outwash.
 - 1 Dutch gunflint fragment, Subtype C. (#95D).
- 14 French gunflints.
- 10 French gunflint fragments.
 - 3 English gunflints, double edged, made on French blades, beeswax. (#110D, #689C and #41D).
 - 2 English gunflints, large.
- 25 English gunflints, intermediate.
 - None small.

Note:

- 14 gunflints not classified due to burning, etc.
 - All Dutch gunflints from Riss outwash (Subtypes A & B), except fragment #95D, beeswax, (Subtype C).
- 1 Dutch found with French gunflint (#41D).
- 4 Dutch found with French gunflint (#838D).
- 3 Dutch found with French gunflint (#364D).
- 1 Dutch found with 2 French gunflints (#647C).
- 1 Dutch found with 1 French gunflint (#523C).
- 2 Dutch found with 3 French and 1 double edged English (?) gunflint of French beeswax (#689C).
- 4 Dutch found with 1 French gunflint (#6060C).
- 1 Dutch found with 1 French fragment and 1 English (?) single edged gunflint on a French blade (#41D).

Comments:

The three possible English gunflints made on French blades, No.'s 110D, 689C, and 41D, deserve particular attention because I doubt whether they are actually of English manufacture. All three of them have a bulb

of percussion and a large flake scar on one side only. Furthermore, the bulb of percussion is not like those normally associated with English gunflints, where they usually appear along the sides of a flat upper face. The three flints here in question are all triangular in cross section and the bulbs of percussion are at the apex of the triangle. Also, all three gunflints have more of a French than English appearance. I believe that we have here three examples of genuine French flints which happen to have small bulbs of percussion, possibly acquired through accident. However, I have marked them as English with a question mark so later investigators can single them out for study.

Whether or not I am correct in my interpretation of the English (?) gunflints, the eight instances where conventional gunflints were found in close association with the Dutch deserves more careful study than I am able to give them here away from the records of the excavation, for it may well be that they present a unique opportunity to determine the overlap in time between the Dutch and French flints.

Conclusions on the Sept-Iles Gunflints:

I see nothing in lots 2 and 3 to change my dating as given for Lot 1. I would say that there were two distinct occupations of the site. The first by the French during the last part of the 17th Century and ending somewhere around 1720. It was later occupied by the English toward the close of the 18th Century or during the first decade of the 19th. If it was occupied from 1760 to 1800 the number of people living there were relatively few in number. Disregarding the three "English" gunflints made from French beeswax chalcedony, we get the following breakdown:

<u>Dutch</u>		<u>French</u>	<u>English</u>			
A & B	C		Large	Intermediate	Small	Fragments
36	3	10	9	106	11	9
8	1	1	6	13	4	0
47	1	24	2	25	0	0
91	5	35	17	144	15	9

The intermediate English flints seem to me to be more fitted for use in either trade guns or carbines than in military muskets. They are too large for long rifles, and I doubt whether the British army would have had an outpost outfitted with rifled muskets at the date indicated. The probabilities are that most of the intermediate English flints were used in trade guns.

The Gunflints from Mingan

126 Dutch gunflints from the Riss outwash (?), Subtypes A & B.
 1 Dutch gunflint, Subtype C, beeswax French, #3641.
 55 French.

3 English, large.
8 English, intermediate.
3 English, fragmentary.

Comments:

Looking at the Dutch gunflints as a group, my impression is that some of them do not correspond with the spectrum of flints which have been identified for me by Witthoft as belonging to the Riss outwash. On the other hand, many of them do. I suspect that the problem is that there are flints from the Riss remaining to be identified and that this is an entirely orthodox collection. However, until we have more examples of known Riss outwash flints, the Subtypes A & B should not be thrown overboard.

For instance, some of the Dutch style gunflints bearing no resemblance to the Riss flints I have and which I would call Subtype A (chert) are:

#1357, brownish grey
#5531, grey white
#5545, solid white. This is its natural color; it has not been burnt.

And there are others about which I have my doubts.

Specific Dutch gunflints which do seem to fit into the Riss spectrum are Numbers 1841, 5481, 5803, 3792, 733, 3441, 801, 2223, and 3641.

The subtype C Dutch gunflint, #3641, is made from regular French beeswax chalcedony, with secondary chipping along both sides and the back. A large flake has been broken out of its upper face.

The French gunflints are strictly standard, of good quality, excellent workmanship, and all made of beeswax chalcedony.

There are a total of 14 English gunflints. In spite of the small number, compared with either the Dutch or the French, they are of interest because of the high quality of workmanship and flint displayed. They are all single edged, as at Sept-Iles, but most of them are neatly squared off at the back.

Witthoft has found (Hamilton, 1960: 74) that during the 1750-60 period the characteristic proportion of Dutch to French gunflints was approximately 50-50 and that the Dutch flints were no longer in use by 1775. The English flints first appear around 1750 but by 1775 only 5% of the gunflints found even on British camp sites are English, the other 95% being French.

With the above in mind, I would think the probabilities are that the 14 English flints are intrusive, for the proportion of 127 Dutch to 55 French indicates an early 18th Century occupation by either the French or the British. Since Mingan lies in French territory of that period, the occupation was obviously French.

I would like to see the provenience of the English gunflints checked to see if it is possible to determine whether they were intrusive or not. Their numbers are: 2129, 5079, 5255, 3412, 5581, 1892, 5485, 1130, 3269, 1121 (?), 799, 5091, 799, 5091, 795, and 5284.

Concluding Comments

These gunflints from Sept-Iles and Mingan may be two of the finest collections of archaeological specimens extant today. Whether they will prove to be so or not will depend upon the condition of the sites, how stratified and undisturbed they were, the care and skill with which they were excavated, and the completeness of the documentary and archaeological records.

Since the records of the excavations are in French, and since it was impractical for me to attempt to evaluate the material through an interpreter and over the intervening distance, I have been forced to treat both collections as though they were surface finds. In a way this has been fortunate for these remarkable assortments of gunflints presented an opportunity for me to test Witthoft's theories on utilizing gunflint types in dating archaeological sites; a problem of primary interest to me at the moment. Undoubtedly, if he had been working with this material, Mr. Witthoft's conclusions would have differed from mine on certain points, but it is to be hoped that I have not done too much violence to his basic thesis.

Before making this analysis I attempted to acquaint the French reader with what has accumulated so far in the English literature on gunflints so that he can carry on with the study independently. Witthoft's findings and theories need to be known more generally among historic archaeologists and used, for that is the only way they can be tested, the exceptions noted, and, if they seem to be proving out, applied.

This paper is, in my opinion, only half written. It remains for Father Levesque and his students to see how accurately my conclusions as to dates and occupations fit the documentary and archaeological facts. I shall await their findings with interest.

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* [Editor's note:] Since published as: "A History of Gunflints,"
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ANALYSIS OF THE GUNFLINTS OF SEPT-ILES AND MINGAN*

Rene Levesque

Introduction

We now pass on to the conclusions that may be drawn from Mr. Hamilton's analysis in the light of the archaeological excavations. First we will give a brief resumé of the principle factors involved in dating historic sites. Then we will study the particular problems raised by Mr. Hamilton's work. Finally, we will undertake the analysis of the gunflints of Sept-Iles and Mingan bearing in mind the preceeding chapters.

Dating

This is a resumé of known factors affecting dating which will be followed by a study of gunflints according to their stratification. First, there are no "Nordic" gunflints at Sept-Iles or at Mingan. Since this type of flint went out of use in the 1650's, we can limit the settlements at Sept-Iles and Mingan to sometime after 1650. This date is confirmed by the appearance of the "Dutch" type at about the same time, which was in use until 1750. It is interesting to note that the latter type was very numerous in both settlements under consideration. There are also some "Dutch" flints of type C which are ordinarily post 1680, which implies that types A and B belong to an earlier period. The French flints appear towards 1680 for Hamilton and 1740 for Witthoft, although the latter admits the possibility that occasionally a French gunflint will appear towards 1680. The English flints make their appearance in approximately 1775 which sets a limit for the beginning of the English régime and for the end of the French régime.

Study of Several Problems

Mr. Hamilton raises several problems in his text which he could not solve without a knowledge of the archives and the archaeological excavations.

Problem 1 - Page 69 (Hamilton's Text)

The author compares pieces 108A and 37A, "Dutch" flints of type C, with a French piece 195-A-a. However, pieces 108A and 195-A-a were found in the same zone, directly behind the two old French buildings G and H which were literally filled with "Dutch" gunflints of types A and B. And, since there were other French flints and "Dutch" flints of type C in zone F between the East-West lines 125 and 155 and the North-South lines 25 to 50, it appears that the building was post 1680. As for pieces 35 and 198A, they were situated some distance from each other - piece 35 was found along the exterior wall to the south of the main building of the Hudson Bay Post in which only English gunflints were found.

* [Editor's Note] This paper was submitted by T. M. Hamilton as a part of his paper.

Problem 2 - Page 69, gunflint #94A

This perfect English flint was found in its natural element - it came from a place where there were many English flints, the section of houses B and D which housed the Hudson Bay [factory].

Problem 3 - Pages 69-70

The author was surprised by the presence of English flints with a double bevel made from French beeswax blades. Are they really English? These flints were situated in the middle of two old houses of Sept-Iles which I suspect were the trading post of Louis-Jolliet and Francois Bissot (houses G and H) because 99% of the flints they contained were "Dutch", types a and b. The flints (classed as English by Mr. Hamilton) were all found at a depth of 6 to 9 inches which is a good deal higher than the level at which traces of the old buildings were found. They could be related to this lower level, or to the debris of house F situated directly behind the earlier buildings. From this it must be concluded that the flints mentioned by the author are French, since the area of houses G and H produced archaeological material of the 17th century.

Problem 4 - Pages 69-70

Mr. Hamilton wondered about the eight cases where normal French flints were found with "Dutch" flints. Amazingly enough, they were found in the area of houses G and H on Sept-Iles! As in the preceding case, they were lying in the top level, more exactly, in the first three inches, except flint 698C which lay at a depth of 9 inches which is still above the occupation level of the French buildings G and H. These gunflints belong then to the house mentioned above, situated behind (house F), unless an upheaval caused them to move from a lower level.

Problem 5 - Pages 71-72

Only one of the gunflints which did not fit into the Riss flints spectrum in the author's collection was found in the main house [A] at Mingan. The flint was found at a depth of 6 inches. The others were found near [French] house G and the Hudson's Bay Post (letter H). The flints which correspond to his collection were all found in the occupation levels of [French] house B.

Problem 6 - Page 72-73

Of all the English flints from Mingan mentioned by Mr. Hamilton, 5 came from [French] houses A and B. It is interesting to note that they were all found no deeper than 6 inches, most were found in the first 3 inches. Thus it is a question of an intrusion of English flints in a French house, but at surface level. The other English flints came from zone H where the buildings of the Hudson Bay Co. stood, and from the path connecting the building of the old Post [Vieux-Poste], close to house G.

Analysis of the Two Settlements

The Gunflints of Sept-Iles

House A

The house dates certainly from the English regime if one considers the evidence of the gunflints found there. Of a total of 24 flints, 23 were English (95%). The only other piece was "Dutch". The final report will confirm this evidence; e.g. the artifacts and the dimensions of the building which correspond to Harrison's plan (1786).

House C

Here, the problem is more complex. There were 54 English flints, 13 "Dutch" and 3 French. The English flints were found in the centre, surrounding 4 "Dutch" flints in trench S-215 and E-170. The number of "Dutch" flints increased near the exterior of the building, although there were an equal number of English flints in the North trench of this house. We are forced to conclude a double occupation. Traces of fire may indicate a building reconstructed on the same site. This suggestion is supported by a thick layer of clay which covers most of the debris. The artifacts lay above and below this clay. (English - 77%, French-Dutch - 25%). Only an examination of the archaeological findings as a whole will produce an adequate solution to this problem.

As for the age of this building, we offer the following hypotheses. The presence of "Dutch" gunflints of types A and B imply a house dating before 1680; this is confirmed by the fact that only 3 French flints were found. On the other hand, the 54 English flints imply a terminal date of at least 1775, which fits very nicely with the posts of the North-West and Hudson Bay Companies. Conclusion: a house existing from 1660 to 1720 approximately, and another superimposed on top; either reconstructed or simply re-occupied until 1775.

House B

This house, situated along the line E-115, may have been the [factory]. The zone itself, according to Harrison's plans, may be divided in two sections: the North part between the lines S-50 and S-200; and the South part between lines S-200 and S-250.

In the South part, the gunflints can be broken down as follows: 47 English flints, 6 "Dutch", 2 French. Thus in this area there was an English fouse - the earth floor of this house corresponds with the dimensions on Harrison's plans. The French and "Dutch" flints found here may simply be articles lost by the first occupants, or they may have come from the house described above.

The part between the lines S-150 and S-200 gave more indications of a French occupation. The gunflints can be broken down as follows: 10 English, 7 Dutch, 1 French; that is 10 pieces of the English régime, and 8 of the French régime. There are several explanations for this co-existence. Perhaps there was an old French building on the site - the stratification would suggest this. It is also possible that the proximity of the typically French zones F, G and H caused this mixture. But it also seems that the presence of the house C which is nearby may have exerted some influence since it contained a similar combination of gunflints. Only a study of all the archaeological findings will give as adequate answer.

House H and G

These two buildings, situated near the bay of Sept-Iles and partially sunk into the embankment surely date from the French régime. We shall first study the building between the lines E-100 and E-140. It contained 5 Dutch flints as opposed to one English and no French. Significantly, there was a similar house found in the Ile-du Havre at Mingan. All the gunflints found in the latter building during the 1966 excavations were Dutch, types A and B. This similarity between the two buildings is confirmed by the artifacts, the ceramics and the dimensions. Thus we have concluded that these two houses in Sept-Iles and Mingan, dating from 1650 to 1700 according to the Dutch gunflints, types A and B, were the two trading posts of Bissot and Jolliet; the post in Sept-Iles was the work of both traders.

House F

This house was found within the co-ordinates S-0 to S-60, E-0 to E-200. The gunflints break down as follows: 8 Dutch, 4 French, 1 English; that is, 12 flints of the French régime and only one of the English régime. It seems that this building was a French house destroyed by fire. Traces of the house would indicate a length of 60 feet and a width of 30 feet (approximately) in the shape of a [bean]. We saw above during the discussion of problems 1, 3 and 4 that the gunflints from this area were Dutch type C and French with the exception of 1 English flint. Thus we can date this house between 1680 and 1775.

House E

This area is English according to the 8 English flints found as opposed to one Dutch and no French. It is likely that this is the "Canoterie" [canoe storage area?] mentioned on Harrison's plan, but the final report may change this hypothesis. The Dutch flint is intrusive and may come from house B. Thus the building must date between 1775 and approximately 1840 when the Hudson Bay Company abandoned its buildings along the present-day Rue Arnaud.

Gunflints of Mingan

Houses A and B

In house A, situated to the West, the following gunflints were found: 5 Dutch, 3 French, 1 English. House B yielded 2 Dutch flints, 9 French and no English. We know then that for the two houses the number of well stratified Dutch and French flints only differs by two units. According to Mr. Witthoft's hypothesis, a 50-50 proportion of Dutch to French gunflints in a given area indicates a date of 1750. Since the proportion found here was not perfect we can assume an earlier date, especially if we take into consideration the Dutch gunflints A and B found in and immediately around the house. Because of the flints of type A and B, the house must be pre-1860, which strongly implies one of the buildings of Bissot's seigneurie.

If we consider the gunflints found outside houses A and B between the houses and the palisade to the South, there are 40 Dutch as opposed to 35 French. Grouping the flints found inside and outside these two buildings, we find the following totals: 56 Dutch and 47 French. But since there are both Dutch flints of type A and B, and Dutch flints type C and French flints, we can conclude that the site was occupied between 1650 and 1755, the latest possible date. These dates agree with those of the trading post.

House C

We shall now consider the gunflints found here and there near the palisade moving southwards. We notice that the Dutch flints were found in greater numbers than French flints in the proportion of 34 to 14, and 16 to 5. From this evidence, we can assume that house C, situated near the palisade is the same age as houses A and B, but that it did not exist throughout the whole French flint period. May we conclude that this house was destroyed before houses A and B which each contained a good proportion of French flints?

House D

There was another house in front of the Philippe Piétasho's house (f) which we have called D. If we rely only on the surface finds and the results of a few trial trenches, all the gunflints were either French or Dutch. It may be a question of another building of the Old Post.

House E

The house situated in front of Abraham Mestekosho's, which we have called E, revealed in trial trenches and surface finds, 10 Dutch, 6 French, and 1 English flint. Thus it is much the same as house A, B

and C. A study of all these houses implies a site with a limit of 1650 on account of the Dutch gunflints, types A and B, and an upper limit of 1775 on account of the French flints and the almost total absence of English flints. From this study of gunflints only, we can conclude that these buildings are those of the old trading post of Francois Bissot.

Conclusion

This concludes our short analysis of the gunflints of Sept-Iles and Mingan. It is remarkable that from one item one can draw valid conclusions that will be supported by archaeological findings and artifacts. I would agree that gunflints are an excellent dating method for historic sites.

In the light of these facts, we shall postulate a few hypotheses. Mr. Hamilton should continue to use the sub-classifications of A and B of Dutch flints. They distinguish perfectly the two old trading posts of Jolliet, both at Sept-Iles and at Mingan. One can also express the opinion that French flints in actual fact did not exist before 1700, as is also the case with Dutch flints of type C. For the moment we shall maintain these two hypotheses.

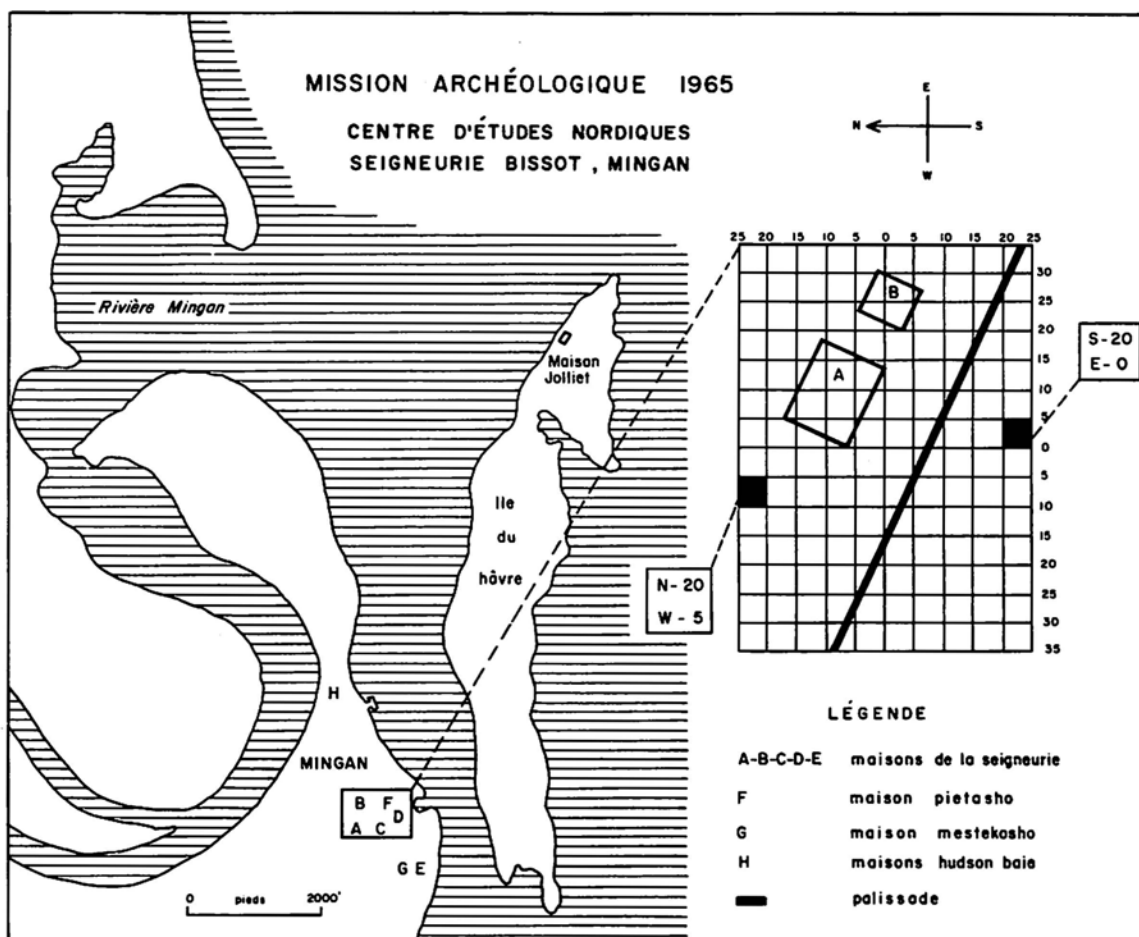


FIGURE 4

MINGAN

MISSION ARCHÉOLOGIQUE DE SEPT-ILES 1963-64
POSTES DE TRAITE BISSOT-JOLLIET & HUDSON BAY
SOCIÉTÉ D'ARCHÉOLOGIE DE SHERBROOKE

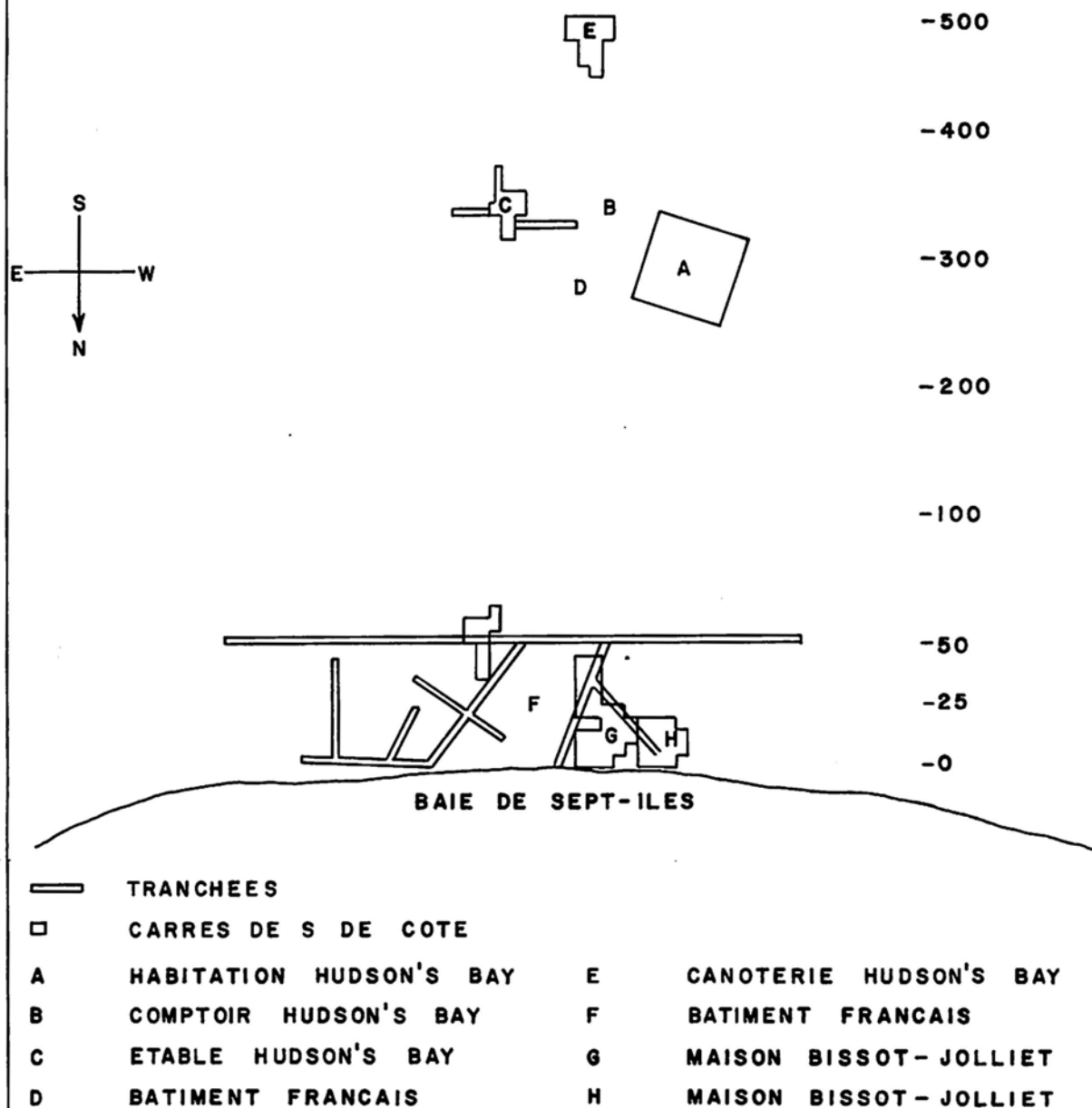


FIGURE 5

SEPT-ILES

MORE THOUGHTS ON THEORY AND METHOD
IN HISTORICAL ARCHAEOLOGY

Clyde D. Dollar

It has been said that although God cannot alter the past,
historians can; it is perhaps because they can be useful to Him
in this respect that He tolerates their existence.

Samuel Butler (1835-1902)
in "Erewhon"

Ladies and gentlemen:

In November of 1967, it was my honor to present to this Conference a paper entitled "Some Thoughts on Theory and Method in Historical Archaeology." Unfortunately, I was unable to be with you in person for that meeting, and therefore had to impose on your good graces by making a taped presentation instead. You no doubt are familiar with the subsequent use of this paper by Mr. Stan South as the subject of the "Historical Archaeology Forum" for this Conference's 1967 publication.

It was my intention to follow this initial paper with some additional exploratory theory work for presentation to the 1968 Conference. To this end, I collected notes and sketched out a rough outline of the presentation--only to have the project sidetracked by my involvement in the making of "A Man Called Horse," a CBS feature film depicting the Teton Dakota of the year 1825. This film is due for nationwide release in the near future.

After a two year lapse, this new paper is here presented for your consideration. I have entitled the presentation: "More Thoughts on Theory and Method in Historical Archaeology." Judging from some of the reaction to my original theory paper, there will be those who might wish that this paper had been instead titled: "Second Thoughts on Theory and Method, etc." On the other hand, I am sure that the Bishop of Wittenberg was genuinely upset with all those nail holes in his church door.

Now, to the subject at hand. The most appropriate point of departure is the summary given in the closing pages of the first "Thoughts" paper. For purposes of clarification, I will quote this summary verbatim:

- 1). It is time to give serious thought to the recognition of historical archaeology as a distinct socio-scientific discipline with a methodology designed to cope with the unique problems encountered during the excavation of historical sites;
- 2). Two of the major research methods used by the historian are the logical processes of deduction and tests for validity, and both of these aspects must be a property of the research at an historical site if such research is to be legalistically and microscopically oriented, and therefore, considered historically valid;

3). The techniques of archaeology (which are not the exclusive property of the discipline of anthropology) are field techniques only, and any 'extended' use of these techniques by the researcher is grounded on a statistical basis too far removed from the possibility of verification to be usable in historical research processes;

4). The concepts of anthropology are oriented toward macroscopic inductive processes and inferential verification, and training in this field frequently does not prepare the anthropologist to cope with the problems faced when researching an historical site;

5). The ten theses are as follows:

i). Since the late 18th century (in America), the number of different physical cultural expressions in the areas of artifact forms and variants within these forms has increased to a point where the subject is extremely complex;

ii). While typological processes, in general, can be applied to any given body of historical site artifacts with a specified spatial and temporal limit, seriation processes, or the attempts to derive temporal data from within a typological pattern of historical site artifacts, have not as yet been proven to produce totally non-distorted historical data, and therefore, must not be used alone in the construction of historical hypotheses;

iii). Every archaeologically recovered artifact from an historical site has two inherent dates; its date of manufacture and its date of deposition;

iv). The date of manufacture for every archaeologically recovered artifact from an historical site implies two separate dates; an 'alpha' date and an 'omega' date;

v). Within the provenience period of an historical site, at least two separate and related time periods must be delineated; the historical period and the 'alter' period;

vi). Every archaeologically recovered artifact from an historical site has two definable locations in relation to the matrix of the site (the 'locative' characteristics of an artifact): the vertical location and the horizontal location;

vii). An archaeologically recovered artifact found on or in an historical site cannot be dated based only on the fact of its being found at that site, nor can an historical site be specifically dated by the artifacts found within the matrix of that site;

viii). The discipline of historical archaeology must be architectural in orientation and reconstructive in both purposes and scope;

ix). A considerable and basic dissimilarity exists between archaeological evidence for structures at an historical site and such evidence for structures at a prehistorical site, and certain archaeological field techniques, if used at an historical site, can actually destroy important historical evidence of an architectural nature;

x). It is a function of the historical archaeologist to find and present evidence, develop hypotheses, and establish facts regarding both site identification and site authentication (2).

I would like to make two changes. First, Iain C. Walker has suggested a better terminology for use in Thesis #5, the words, 'subject' and 'non-subject', to replace the terms 'historical' and 'alter' which I had originally proposed to designate the two major time periods at an historical site (3). I must again emphasize, and I'm sure that Walker will agree with me, that these designations do not imply that one of the periods should be given more research importance than the other. In fact, the express purpose of the formulation of Thesis #5 was to suggest that an equal research emphasis must be given to all periods of a site's history.

The second change is again one of terminology and involves my use of the word 'authentication' as opposed to 'authentication'. This was a verbal error on my part, done with no conscious attempt to create a new word. Stan South called my attention to the error (along with several others) and I requested that Stan make the necessary changes to correct the spelling of the word. Ed Larrabee, in the meantime, commented on the use of the incorrectly spelled word (4) and thereby necessitated leaving the "pleonastic 'fi'" as it was. My syllogistic reply to this point (5) was 'tongue-in-cheek', as I'm sure everyone recognized. May I settle this: the word is "authentication" and not "authentication".

In this present paper on theory and method in historical archaeology, I will deal with only two main areas; use of statistics and the evaluation of artifacts.

It is extremely important that we reduce the possibility of semantic error by understanding the terms which we will be discussing. In the first "Thought" paper, I defined typology as the process of arranging into groups those artifacts with a significant similarity of observable physical characteristics. I then defined seriation as the process of arranging these typological groups into certain patterns or orders in an attempt to determine temporal sequences or relationship. The observation was then made that the use of these seriation techniques seemed to be producing distorted information at historical sites, and I indicated several instances of this based on personal observation and knowledge. In the discussion which followed, I suggested that there were three reasons why this was happening:

- 1). There was a statistical invalidity in the seriation process;
- 2). There is an ever present possibility of a subjective analysis within the basic data gathering process;

3). The information derived from use of seriations at prehistoric sites has not been subject to verification, and therefore, seriation processes are only hypothetical in their validity. It stands to reason, therefore, that before such processes can be used at historical sites, and their results accepted as historical data, the hypothesis that seriation processes are valid must be subjected to further verification (6).

It was unfortunate that some felt the original "Thoughts" paper was an attack against the entire field of anthropology and all those who were associated with it. I trust that the intervening years and clearer heads have dispelled this notion. There were also some who understood my statements regarding the use of statistical processes for research at historical sites as being a blanket condemnation of the use of any and all statistical processes at any site. In this respect, I trust that the careful reader will be able to make a more rational judgement.

We must again define terms. Statistics is a body of methods for obtaining knowledge (7). It is a word that can be applied to any fusion of logic and numbers, be it the simple expedient of numbering pages of a report or calculating the most intricate deep space navigation. One of the techniques used by the field of statistics for obtaining portions of knowledge is the use of probability theory, which is the study of mathematical models of random phenomena (8). Albert Spaulding says essentially the same thing when he defines statistical inference as the process of "making the best possible decisions in the face of uncertainty about numerical matters...." (9). Probability theory makes use of certain specialized word functions. Some of these we must mutually understand. A population is "the totality of all possible observations of the same kind" (10). A sample is the observable portion of any given population, frequently referred to as "the data". Sampling variability refers to the pattern of this sample and the relationship of this pattern to the population from which the sample originates. One particularly important type of sample is the random sample, which can be defined as follows:

"'Random' as used in statistics is a technical word, it has a meaning different from the one given it in popular usage. When a sample is called 'random', this describes not the data in the sample, but the process by which the sample was obtained. Thus, the randomness is a property not of an individual sample but of the process of sampling..." (11).

To reduce the bias in the sample is assumed by many to be the main reason for making a random sample. Of more importance, however, is the fact that the pattern of sampling variability for any population is known if, but only if, the sampling is random (12). Spaulding, in 1959, stated flatly that he knew of no archaeological sample that he could consider as random (13). To my knowledge, there are still none as of this date.

Let us assume an hypothetical situation. Suppose an archaeologist has found X-number of fragments of the same general type of an artifact. He has found these within (as an example) a five foot square excavated to a depth of five feet. Furthermore, let us assume that from this body of artifacts, seven groups can be coalesced based on their characteristics. These groups our hypothetical archaeologist will designate Type A, Type B, Type C, etc., until all seven of his types are labeled.

In devising a typology, an archaeologist arrives at a specific statistic which is a function of the observations in a sample designed to estimate a parameter of the population from which the sample was drawn--that is, if it is a random sample. The typology, by definition, is based on similar or identical physically observable characteristics of any given body of artifacts. This body of artifacts can only have a spatial relationship at this stage, and therefore, this relationship is measurable (in terms of inches/feet, or centimeters/meters, etc.). Please note that this body of artifacts can also only be a sample, and not the total population (unless the archaeologist wants to define the artifacts found in any given area as the total population of any such artifacts; this is as absurd as picking up a handful of sand on the beach and defining this amount as the total population of all the sand on that beach). Furthermore, please take note of the fact that this body of artifacts does not represent a random sample, and therefore, cannot be used to define the normal distribution of the population or the probability of occurrence within the population. A typology is not created to perform this function, and is therefore, not invalidated by lack of statistical cohesiveness. A typology is simply an ordering of materials based on their relative characteristics and is therefore useful in data collecting and description. A seriation, or the arranging of these typological groups into certain patterns or orders in an attempt to determine temporal sequence or relationship, is quite another matter, however. Temporal relationships between artifacts, and therefore typologies, is assumed on the basis of Lyell's Principle of Stratification, providing there has been no disturbance of the stratigraphy since its deposition. I take no issue with this assumption and thereby grant that there is a temporal relationship, but I am not convinced that there is a valid and definable statistical relationship from an archaeological standpoint. Within each set of data (i.e., sample, or, in our hypothetical case, each of the archaeologist's types), we see the actuality of occurrence, and the assumption is made that this actuality of occurrence has a relationship of some nature to the total population. The exact nature of this relationship is unknown, and, as I have tried to point out in the preceding paragraphs, not statistically definable based only on the archaeologically obtained sample. This situation means that the archaeologist (or the statistician) has no way to determine the boundaries of probability occurrence (i.e., the temporal beginning or ending of those observable characteristics that comprise the sample). These boundaries of probability occurrence are referred to as the sigma limits or the terminal points of the deviation within the population's normal distribution. For archaeological purposes, the term 'sigma limits' can be given a temporal application. There is therefore no way to determine a meaningful confidence of probability, i.e., that the actuality of occurrence within any given sample will also occur for the population.

Normal distribution of any given population can be determined within a statistical process if it can be physically measured, or if it can be inferred from previous statistical processes using a similar and valid set of data (sample).

Once confidence is established for the normal distribution of a population, then it is possible to determine the probability of occurrence within similar populations under identical sets of circumstances. If a population is homogeneous and the sample is random, the probability of occurrence and distribution curves for any other similar population will also be similar. It is extremely doubtful that this has ever occurred in an archaeological

deposition situation.

Now, let's return to our hypothetical archaeologist and his seven types. Assuming that the matrix from which the artifacts came could be divided into only three different and distinguishable levels, and for purposes of clarification, let's call these Levels One, Two, and Three, with Level One being the uppermost strata and Level Three being the bottom strata. Types A, B, and C were found in Level Three; Types B, C, D, and E were found in Level Two, and Types D, E, F, and G were found in Level One. On the basis of this seriation, our archaeologist will assume that Type A is the oldest of all his types, that Types F and G are the latest, and that there is a mixing of the other types during a 'middle' period. Several other conclusions might be drawn or inferred from this seriation.

This is an oversimplification, of course, but I ask that you bear with me for the purposes of this analysis. Obviously, there are other factors that would need to be considered in this case, such as the quantity within the individual types and possibly the inherent characteristics in the artifactual material, but for purposes of our discussion, please assume only the data here given.

We have already discussed the fact that an archaeological excavation does not constitute a random sample, and that therefore, the sampling variability of the population cannot be known. This means that we have no way to statistically compute either the sigma limits or the normal distribution for that population. In archaeological terms, it means that we have no way of knowing what the artifact distribution within any given type actually is throughout the entire site (i.e., the total population). This uncertainty introduces a considerable probability of error into any comparison of types (seriation). By seriating types, the individual segments of which were obtained through the use of standard archaeological techniques, we run the serious risk of constructing comparisons using totally dissimilar distribution curves. In short, there is a distinct probability that the construction of seriations actually compares apples with oranges and gets bananas as a statistical result (14).

I have said before, and will here say again, that there is a temporal relationship through the locative characteristics of any two artifacts (15). The problem is to validly define this relationship, and the only way to prove this validity is to construct seriations, the result of which can be verified using data external to the seriation itself. Clearly, there are situations when it is impossible to verify the results of the seriation. In these instances, the data obtained from the seriation can only be hypothetical. When working in prehistory, it is frequently necessary to accept these hypotheses derived from seriation processes as, at least, 'working data' or quasi-factual. The extent to which these hypotheses are given factual status depends on the credibility standards of the worker and the situation in which these hypotheses are used. Seemingly, the older the material comprising the seriation, the more statistical latitude is accorded to the validity of the results. This is one of the reasons why Sir Flinders Petrie's work with pottery in the Near East, done some seventy years ago, is still useful. Because he successfully produced a pottery chronology through seriation, Petrie's work has formed the theoretical basis for North American seriation (16). This

is most unfortunate. Aside from the obvious fact that his work was done under vastly different cultural conditions, excavation techniques, etc., I would recall to your mind that his seriations, and resulting chronologies, etc., are based on an absolute chronology derived from king lists and grave inscriptions, and he made very limited use of undated sites (17).

During the past decade, the growing interest in the application of quantitative methods to the field work of archaeologists has led to widespread use of seriations. Such use has developed the feeling, perhaps unconscious, that the application of quantitative methods to archaeological material was the key to Instant Truth. This tendency has reached such a climax that we now have archaeologists enthusiastically constructing seriations for everything that doesn't move at a site, regardless of the age either of the site or the material. Spaulding, and others, specifically warned against such tendencies (18). Ladies and gentlemen, I tell you without reservation that this approach is not valid for use at historical sites, and I urge you to stop using it unless you can verify the results of your seriations using external (i.e., historical) data.

Why are seriations usable in prehistory and not in history (as defined on the North American continent)? The answer lies in a function of Time.

My argument against the use of seriations when researching most historic sites in North America is based on self limiting factors within the framework of statistics itself. The temporal unit of a site is definable as a population from the standpoint of physical measurement (minutes, days, years, etc.). But for archaeological purposes, it takes on the nature of only a statistical sample--and again, not a random sample.

As an example, for a 6000 year old site, there could be 6000 temporal units defined. From a purely statistical standpoint, all of the temporal units within the total age of the site could be defined without regard for the events which occurred within the total age. In this sense, the temporal scale becomes a population. For example, in Old World sites, the sigma limits of this population could be established using historical data (kings lists, stelae dates, inscriptions, etc.). For prehistoric sites, where no such data exists, the temporal scale becomes an 'unknown', or undefinable quantity. However, it is not temporal units alone that we are seeking to recognize, but rather the events which occurred within those units. The finding of an artifact represents only the findings of an actuality of occurrence which took place at some undefinable point along the temporal scale. Thus, artifacts represent only a sampling of the total temporal scale, and since they do not represent a random sample, the population cannot be determined with any accuracy.

One of the ways to overcome this limitation is to use methods of dating with definable temporal scales. One such method is the Carbon-14 process. For a 6000 year old site, the error factor, expressed on a C-14 scale, might be only plus/minus 400 years, or approximately 12% of the total temporal period involved. This p/m factor is relatively insignificant when compared to the total temporal period at the site. By its very nature, this p/m factor is generally not recognizable from the archaeological standpoint. However, because the C-14 test, and other such physical/chemical tests, are based on a definable (and hence measurable) scale, their results (all else being equal) furnish the archaeologist

with a valid tool for dating older objects and sites--provided that the temporal span of the site being researched is long enough to allow the p/m factor to remain in the vicinity of 10%-12%.

The picture changes considerably when we start applying these techniques to a very restricted temporal scale. For a research situation that is, for example, only 150 years old, the temporal period, as defined by artifacts alone, is still only a sample of the temporal scale and not the total population. Unfortunately, the existing physio-chemical dating tests are not sufficiently refined as yet to be of assistance within this short temporal span. In addition, the influence of a rapidly changing technology (such as what occurred during the 18th and 19th centuries) caused events which directly affected the placement and type of artifacts to be found in a 'late' situation to occur much faster than similar significant events which would have taken place at an 'early' site (19). The increased complexity of these events with the 'late' site's temporal span would cause the temporal scale to become even more of a statistical sampling (keep in mind this is still not a random sample). In fact, under these conditions, the 'late' site temporal span takes on the characteristics of a highly restricted sample, and hence becomes quite unreliable from a statistical standpoint--and therefore, also from the standpoint of the researcher using the data derived from statistical techniques. This increase in probability error causes the p/m factor to be much greater than what would normally be considered 'insignificant' from the standpoint of percentage relation to the overall temporal scale. In short, a 12% p/m factor for a 6000 year old site might be an 'insignificant' 400 years, but the same percentage p/m factor for an 150 year old site becomes a rather significant 17.5 years, or a sigma limit of 35 years. And at an historical site, a great deal can take place in this period of time. These figures are based on the assumption that each of the two dating situations, both the 'early' and the 'late' site, and their respective p/m factors, are directly proportional to each other. I strongly suspect that this direct proportionality is not the case, as the increased number of archaeologically definable events (i.e., the actuality of occurrence) and the relative greater speed with which they occur in a 'late' situation, would tend to increase the probability of error within the statistical situation. This would, naturally, also increase the p/m factor for the 'late' site.

I know of no way to mathematically calculate this increase in the p/m factor for late sites. Through observations made in the course of correlating archaeological and historical data for specific sites, it appears to me that the disparity between dates derived from historical source materials and those derived from seriations applied to archaeological materials, range from about 25 to 60 years in error (over a time span of approximately 150 years). In other words, a p/m factor of from about 17% to as high as 40% has been noted. It was on these observations that I based my remarks in the first "Thoughts" paper that the use of seriation techniques in historical archaeology research situations has

"...not as yet been proven to produce totally non-distorted historical data and therefore must not be used in the construction of historical hypotheses -- unless, of course, exteriorly known data can be used as corroborative evidence." (20)

So much for seriations. Now let's turn our attention to the evaluation of an

artifact as evidence in historical archaeology research.

A considerable amount of writing in the first "Thoughts" paper was devoted to discussing the attributes of an artifact (Theses #4, 6, & 7). We cannot escape the fact that artifacts are evidence, and, as such, are subject to certain limitations and specific uses. Artifacts found at an historic site can be said to be historical evidence in that their form, characteristics, and, indeed, their very presence at the site has been influenced, even dictated, by the profound improvement in communications which, by definition, separates history from prehistory. Being therefore historical evidence, artifacts are subject to the same general qualifications, rules, and uses applied to other types of historical evidence (21).

A. L. Kroeber once remarked that the writing of history was perhaps the oldest of scholarly pursuits (22). This has given those engaged in this pursuit ample time to at least attempt to perfect certain techniques for using evidence. The main difference, aside from the 2400 years of time, that separates Herodotus from Toynbee is the improvement in the use of historical evidence of the latter over the former. The most significant improvement in methods to evaluate historical evidence took place centuries ago. This was the recognition that there are two major divisions of such evidence, each with its own relative 'weight' of probable validity.

These two major divisions are: primary sources material and secondary sources material. Primary material is that evidence based on an observer's personal and first-hand knowledge of a particular historical event. This is somewhat equivalent to legal evidence classed as Personal Knowledge Evidence. Secondary material is that evidence based on information gained from other than personal and/or first-hand knowledge of a particular historical event. This is somewhat equivalent to the legal definition of Circumstantial Evidence. Tertiary material is built on secondary evidence and is usually not considered one of the major evidential divisions. It is roughly equivalent to Hearsay Evidence in a courtroom (23).

No historical evidence is ever accepted by the research historian at face value--or at least should not be so accepted. The validity and applicability of any historical fact must always be questioned and evaluated, and there are numerous evaluative principles to be applied to any given historical fact. Primary source material is usually accorded more weight than secondary material, but by no means is this automatic. Secondary source material, on the other hand, is seldom accorded the explicit weight of primary evidence. There are numerous questions and causal relationships which can and should be applied to each piece of primary evidence, and if the validity of primary evidence is so suspect, then secondary evidence is even more so.

Frederick Jackson Turner once defined History as:

"...all the remains that have come down to us from the past, studied with all the critical and interpretative power that the present can bring to bear on the task." (24)

This definition, written in 1891, is still acceptable to the research historian,

and would make a welcomed addition to the theoretical repertoire of the historical archaeologist.

The assumption has been made in the discipline of Anthropology that all artifacts found at a site have been primary source material. This same assumption has been transferred en masse to the archaeology of historical sites. I submit to you that, on the whole, this is a fallacious assumption. It is true that artifacts found at an historical site are primary to the event that created the condition in which the researcher found the site as a whole, but they are primary only to this event and secondary to the other events that comprise the history of the site. This means that their presence in any specific location within the site must be questioned and stringently evaluated. There are exceptions, of course, but these exceptions tend to prove the rule. Postholes, for example, would be primary to some historical event that occurred at a site. The problem for the researcher is to identify the event. The same is true of stratigraphic depositions of soil. These are primary to a specific historical event or phase at a site. Artifactual debris found within the stratigraphy does not alone define the temporal relationship of the strata to the history of the site. Such material is primary to the strata, but secondary to the history of the site.

Artifacts, such as ceramics, nails, glass, leather, buttons, and the dozens of other types of debris that are the residue of human existence, are infrequently found in primary association with the historical event that the archaeologist should be attempting to discover. As secondary source material, artifacts are evidence that a particular event did happen, but they are not evidence of when it happened. What I am suggesting is that artifacts actually have little evidential value for specific historical events unless they can be proven to be primary source material to that specific event.

Based on the material in the foregoing paragraphs, I would like to formulate a new thesis to be added to the other ten presented two years ago in the first "Thoughts" paper:

Thesis #11: Artifacts found at an historical site must be assumed to be secondary source material unless, through circumstances inherent in the characteristics of the site, they can be proven to be primary.

At first glance, this new thesis might seem to be highly restrictive, but in its application, I think you will discover an important new technique for analyzing artifacts from an historical site.

In closing, I would like to express my personal gratitude to Mr. Stanley South for the time and effort he expended in preparing the 1967 Historical Archaeology Forum. And to the ladies and gentlemen of this Conference, I extend my sincere best wishes for success in your future endeavors, and I thank you for your attention and consideration.

Good day.

FOOTNOTES

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3. Dollar, ibid., Iain C. Walker's Comments, pp. 111-112.
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5. Dollar, ibid., Reply to Larrabee, Point #21, p. 183.
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7. W. Allen Wallis & Harry V. Roberts, Statistics, A New Approach, Crowell-Collier Publishing Company, Brooklyn, N. Y., 1963, p. 5. This book makes an excellent introduction to the field of statistics.
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11. Wallis & Roberts, ibid., p. 115.
12. Wallis & Roberts, ibid., p. 116.
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14. For related discussions (pro & con), see Robert Stover, The Nature of Historical Thinking, University of North Carolina Press, Chapel Hill, N. C., 1967, pp. 16 ff; Gordon R. Willey and Philip Phillips, Method and Theory in American Archaeology, University of Chicago Press, Chicago, 1958, pp. 23 ff; James Deetz, Invitation to Archaeology, Natural History Press, Garden City, N. Y., 1967, pp. 26 ff; Professor Pittioni's remarks following Spaulding's "Statistical Description", pp. 90 ff; George Kubler, The Shape of Time; Remarks on the History of Things, Yale University Press, New Haven, Conn., 1962, pp. 77 ff; and the Recommendations on p. 357 of Heizer and Cook, The Application of Quantitative Methods.
15. Dollar, "Some Thoughts on Theory and Method", discussion of Thesis #6, pp. 19-21 and p. 29.

16. Deetz, Invitation, pp. 30-33; Robert F. Heizer and John A. Graham, A Guide to Field Methods in Archaeology, the National Press, Palo Alto, California, 1967, pp. 172-173.
17. For a penetrating and thoroughly "British" view of both Petrie's work and chronologies as a whole, see Sir Mortimer Wheeler's Archaeology From the Earth, Pelican/Penguin Books, Baltimore, Md., pp. 29-30 and Chapter 3, pp 38-55.
18. Spaulding, "Statistical Description", pp. 84 ff.
19. Dollar, "Some Thoughts on Theory and Method", discussion of Thesis #1, pp. 13-15 and p. 28.
20. Dollar, ibid., p. 15.
21. Carl P. Russell, Firearms, Traps, & Tools of the Mountain Men, Alfred A. Knopf, N. Y., 1967; Appendix A of this work, "Historic Objects as Sources of History", pp. 387-401, should be required reading for both students and researchers in the field of Historical Archaeology.
22. A. L. Kroeber, An Anthropologist Looks At History, University of California Press, Berkeley, 1963, p. 153 (Chapters Ten and Eleven will also be of interest).
23. For further discussions of historical evidence, see: Stover, The Nature of Historical Thinking, pp. 162 ff; Kubler, The Shape of Time, pp. 39 ff; William H. Dray, Philosophy of History, Prentice-Hall, Inc., Englewood Cliffs, N. J., 1964, Chapters Two and Three, pp. 4-40; Wood Gray, et. al., Historian's Handbook, Houghton Mifflin Company, Boston, 1964, pp. 152 ff; Carl L. Becker, "What are Historical Facts?" in The Philosophy of History in Our Time, edited by Hans Meyerhoff, Doubleday & Co., Garden City, N. Y., 1959, and numerous articles and essays in Fritz Stern (editor), The Varieties of History, Meridan Books, Inc., N. Y., 1956.
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PART 2

HISTORICAL ARCHAEOLOGY FORUM

Centered Around a Paper on Salvage Diving Operations

by

Stanley South

Prompted by a Paper on Salvage Diving

by

Lee Spence

INTRODUCTION

HISTORICAL ARCHAEOLOGY FORUM

This third HISTORICAL ARCHAEOLOGY FORUM is devoted to the subject of underwater archaeology, a subject of particular interest in recent years as more historic wrecks and sites are located beneath the rivers and lakes, and off shore on the continental shelf. The work of the Smithsonian Institution and the University of Pennsylvania in the Mediterranean Sea, diving at Port Royal, the 1715 Plate Fleet wrecks in Florida, the diving operations carried out by the Canadian Historic Sites Service, recovery of fur trade goods at portage sites, as well as numerous wreck operations of the Civil War period have all contributed toward an increased interest in underwater wrecks and sites. This interest has manifested itself on the governmental level in legislation aimed at defining the relationship that exists between the state and federal governments and individuals and organizations diving on and salvaging cargo and treasure trove from these unique time capsules.

At the Ninth Annual Conference on Historic Site Archaeology held in Knoxville, Tennessee, in November, 1968, a paper on "Salvaging the Cargo of the Mary Bowers" was presented by Lee Spence, President of Shipwrecks, Inc. of Charleston, South Carolina. This paper was primarily a design plan for work to be carried out in the summer of 1969. The completion of that work schedule and the subsequent division of the artifacts through the office of the State Archaeologist at the Institute of Archeology and Anthropology at the University of South Carolina prompted my writing a paper entitled "A Comment on the Relationship Between the State and Salvage Diving Operations", which was presented at the Tenth Annual Conference on Historic Site Archaeology held in Macon, Georgia, in November, 1969.

This paper was selected as the springboard for the third HISTORICAL ARCHAEOLOGY FORUM, and a number of individuals involved in underwater archaeology projects were asked to contribute their papers, comments and suggestions on this topic as stimulated by the pivot paper, or by their own involvement in underwater archaeology projects. These contributed papers are presented here in Volume 4 of The Conference on Historic Site Archaeology Papers as the third HISTORICAL ARCHAEOLOGY FORUM. Lee Spence's paper was withheld from publication in Volume 3, so that it could appear here to provide background perspective for the pivot paper.

Thanks are expressed to those contributors who have taken the time to participate in this forum.

Stanley South, Chairman
HISTORICAL ARCHAEOLOGY FORUM
The Conference on Historic Site
Archaeology

SALVAGING THE CARGO OF THE MARY BOWERS

Lee Spence

Perhaps I should begin my paper with an explanation of why I, rather than a trained archeologist, am in charge of the salvage of the English built blockade-runner, Mary Bowers, sunk off Charleston harbor during the War Between the States.

I am like many of the amateur archeologists and arm chair treasure hunters that you must surely know. I dream big. When I was 12 years old, I dreamed that if I was real lucky, I could find an old shipwreck. It was my belief that I could contribute to archeology and that I might even receive a little recognition for it.

I was wrong because I not only found one wreck but I found a dozen and then some. They date from before the American Revolution to shortly after the War Between the States.

Finding that I had big dreams, but no money, real experience, or legal right to salvage these vessels, I contacted various people and agencies in the State and Federal governments to try to secure the things that I lacked. You may have noticed that few people are willing to donate the substantial sums required to carry on a purely scientific project. I failed to secure the help that I sought.

I wrote to an acquaintance of mine, who is a man of great distinction and world renown. My letter explained that I was having problems. I sought his advice on what I should do next. My letter brought an immediate reaction. By return mail came the following reply, and I quote: "Don't touch that wreck until you have established ownership. Until you get the ownership, do not get any publicity at all. Otherwise, some bloody historical society or someone else will jump on the bandwagon and grab your wreck."

I gathered from what he said that this one wreck that I had mentioned to him was not only valuable historically but materially as well. You and I both know that an archeologist wouldn't or, at least, shouldn't get rich from his work. Everyone knows that archeologists are supposed to be poor. I had been told, more or less, that I was not cut out to be an archeologist. I just was not cut out to be poor. It seemed that I had started off on the wrong foot I couldn't do anything right.

Now, all kidding aside, what would you have done with the advice, if you had been in my place? Would you have kept your motives pure? Would you have immediately turned over all of your research to the nearest archeologist? I did not.

I started thinking. I knew that several of the wrecks had cargoes of over 500 tons each. Now, try to think for a minute what amount of money you could get for the intact items that you find on one of your digs. Now, think of their value as blank number of dollars per pound. Now, multiply that

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figure times 500 tons. Remember 500 tons is 1,000,000 pounds. All right, that amount is for one wreck. Are you impressed? If you are, maybe you are in the wrong field.

I formed a corporation with a shrimper, Walley Shaffer and a lawyer, George Campsen. Walley had the boats that I needed, and Mr. Campsen had the legal knowledge that I lacked and for which I could not afford to pay. Shipwrecks, Inc. succeeded in getting, with the aid of the South Carolina Justice Department, a law passed through the South Carolina legislature making it illegal to disturb an archeological site or remove artifacts from State waters without a State granted salvage permit.

On Friday, September 13, 1968, the State of South Carolina granted Shipwrecks, Inc. the first archeological salvage permit in South Carolina history. Under the terms of the law, South Carolina can retain any or all of the material recovered during salvage operations. The State will retain a substantial amount of the material recovered. The State plans to use this material for study and display purposes in South Carolina colleges and museums and to trade with other museums if it needs to do so. The State basically wants to achieve the same benefit from the salvage of these wrecks as it would if it was sponsoring the entire operation with its own funds. An operation expected to require over one hundred thousand dollars worth of equipment consisting of: a 72 foot boat, a 52 foot boat, water pumps, compressors, survey equipment, heavy and light weight diving gear, water jets, air lifts, flotation gear, a small barge, photographic equipment, lights, and communication equipment. A warehouse must be rented for storage of equipment and artifacts. A preservation laboratory must be equipped and staffed. Divers must be paid. Insurance must be bought. Records must be kept. All of these raising the total amount involved to over a quarter of a million dollars. The State is the only guaranteed winner.

One of the ships that Shipwrecks, Inc. had the opportunity to salvage under the State permit was the wreck of the Mary Bowers. It is because of this that I am speaking to you today, both as an interested amateur archeologist and as president of Shipwrecks, Inc.

Now, I will give you a little of the factual information available on the Mary Bowers.

The Mary Bowers was built and engineered by W. Simons and Company, London Works, Renfrew, Scotland. It was a side-wheel two-piped, schooner-rigged, iron steamer. The Mary Bowers was: 220'0" long between perpendiculars; 226'0" overall breadth; extreme 25'0"; depth of hold 10'4.5"; depth moulded 10'6".

The Mary Bowers was the sister ship of the Stormy Petrel and said to be the counterpart of the Elsie. (The Elsie was a swift steamer that was lost in the Gulf of Mexico.)

A newspaper clipping contained in Report No. 86, June 18, 1864, from the United States consul in Glasgow stated:

"Trial trip - the new paddle steamer Mary Bowers proceeded down the river on Thursday, to adjust her

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compasses and test her machinery, which worked very satisfactorily. At the tail of the bank she encountered the new Iona, the Mary Bowers being slightly ahead, which distance she maintained and considerably increased in an exciting race to Garelochhead."

Consular Dispatch No. 18, from Glasgow, dated January 18, 1865, states that the Mary Bowers was a vessel of over 500 tons and 200 horsepower. The Stormy Petrel, the sister ship of the Mary Bowers, is reported in various accounts to have had oscillating engines of 180 nominal horsepower. It has not yet been determined whether or not the Mary Bowers was engined in a similar manner, although it does not seem likely that she was.

The construction plans for the Mary Bowers list her tonnage as follows: weight of engines, 65 tons; weight of boilers, 45 tons; weight of hull, 270 tons; dead weight, 250 tons; for a total of 750 tons including the weight of the cargo.

The Report of John Downes, United States Navy, Commander of the United States Ship R. R. Cuyler, dated August 14, 1864, and sent to Acting Rear-Admiral S. P. Lee, Commander of the North Atlantic Blockade Squadron at Beaufort, N. C., reads as follows:

"I shall take the opportunity of transmitting to you my report of my last cruise, and the chase of a large sidewheel, two-smokestack steamer, which I was obliged to abandon running short of coal, though not until I had made him throw overboard a quantity of cotton, of which I picked up 52 bales."

The London Times of 10 September, 1864, gives further information on this incident:

"Bermuda, August 18. The Mary Bowers from Wilmington, which arrived here on the 13th August, reports having thrown overboard 60 bales of cotton, having been chased by the R. R. Cuyler, Federal gunboat, which vessel picked them up."

The next phase in the story of the Mary Bowers was covered by both the Confederate press and the later government printed Official Records of the War of the Rebellion. In Volume 15, Series 1, of the Navy Records, one finds the following reports concerning the destruction of the Mary Bowers:

U.S. Frigate Wabash
off Charleston, S.C.
September 1, 1864.

Sir: I have the honor to inform you of the total loss of the English blockade runner Mary Powers. She ran ashore some time during last night between Rattlesnake shoals and Long Island. Only one person was rescued from her, by the Wamsutta and the Sweet Brier. She

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hailed from Glasgow, last from Bermuda, bound into Charleston, and probably struck on a wreck, as she lies in three fathoms of water, about three quarters of a mile from shore. I am, sir, very respectfully, your obedient servant.

J. De Camp
(Captain.)

Captain J. F. Green, U. S. Navy
(Senior Officer present)

P.S. - The Mary Powers is a side-wheel steamer and is a new vessel.

U.S. Frigate Wabash
off Charleston, S.C.
September 1 1864.

Sir: Your letter of this date is just received. Respecting the wrecked blockade runner, she proves to be the English sidewheel iron steamer Mary Bowers, and not Powers, as stated in my letter this morning.

Richard Jackson, a boy about 16 years old, was the only person found on board of her. He appears to know little or nothing of the vessel, except that she was to take cotton from here to Halifax. He states that he knows of no cargo being on board. The vessel is undoubtedly lying on the wreck of some vessel as there is deep water all around her. Her bell, marked 1864, her binnacle and compasses, and two kedge anchors are saved; also a quantity of liquor and a few signal flags. The liquor has been put in charge of the surgeon of the ship. No documents of the slightest importance have as yet been discovered. I am, sir, very respectfully, your obedient servant,

J. De Camp
(Captain.)

J. F. Green
(Senior Officer present)

The Charleston Mercury, Friday, September 2, 1864, carried this account of the loss of the Mary Bowers.

"Loss of a blockade runner. - The fine new steamer Mary Bowers, Captain Horsey, from Bermuda, with an assorted cargo, had the misfortune to run upon a sunken obstruction about a mile from the beach of Long Island, near this bar, at an early hour on Thursday morning. The cause of the accident appeared to be the remains of a wreck, and as the Mary Bowers was coming rapidly for the

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harbor she went on to it with such force as to make immense openings in her bottom, and caused her to sink in a few minutes; most of the officers and men saving only what they stood in.

The Mary Bowers was the property of the Lamar Company. The officers and crew reached this city yesterday.

In our study of the wreck of the Mary Bowers, we are concerned with three different wrecks. These wrecks all lie within 250 yards of each other. They are as follows: The Georgiana, carrying 14 cannon and carriages, two Whitworth guns, four Blakely guns, cases of Engield rifles, medicines, and merchandise; the Constance, carrying a very valuable cargo mostly on government account; and the Mary Bowers, carrying a cargo identified by the U. S. Navy as coal and gunny bags, but reported by the Confederate press to be an assorted cargo.

We found what appeared to be two wrecks lying at angles to each other. The wreck that we believe to be the Mary Bowers seemed to be in very good condition. The deck had rotted away, and the wreck was partially buried in the sand. Toward the stern, the wreck rose about seven feet from the bottom. At the bow, the wreck was barely visible, and in places only a few inches of iron stuck out of the sand. In our blundering attempts, we did manage to find cargo. In the ships hold lay what appeared to be tons of artifacts. There were stacks of plates, cups, saucers, jars, and bottles. The sea had taken its toll on the top cases. The wood was rotted away leaving the contents of each case open to the destructive forces of nature. But under this top layer, we were later to find that even the wooden cases were undamaged. In about 15 minutes, I brought 65 blue-edged plates to the surface. Only 25 plates had escaped the ravages of time. I was amazed that the plates had survived at all. They had come from the part of the wreck that seemed to be in the worst condition. My hopes for finding a largely undamaged cargo soared.

Afraid of destroying material that had survived a shipwreck and a hundred years on the ocean floor, we stayed away from the wreck. For the next two months, Drew Ruddy and I slowly taught ourselves the secrets of diving blind. We learned to work underwater without wandering from place to place. We found that we could keep our bearings and return to the same place without fear of losing our way or wasting our time. Furthermore, we were able to draw diagrams to explain to each other how to get to a spot that we had never been before. I finally felt that we were ready to go back out to the wrecks. I firmly believe that it would be impossible to find a more competent diver than Drew.

We returned to the wreck Sunday, April 28, 1968, for our fifth dive. In less than three hours, we had located and brought to the surface dozens of items - saucers, cups, jars, beer mugs, plates, and bowls. We had found cases of material to raise on future dives; enough to convince us that the wrecks were really worthwhile.

Our dives in September were spent in surveying and marking the wrecks. The wreck of the Mary Bowers is iron and the beams that supported the wooden deck will serve as markers for every three-foot interval. Each beam will be marked with a large colored number and a length of nylon cord to show the divers where they are. The numbers can be used on days with good visibility,

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and the cord, coded with knots, can be used on days with zero visibility. The level at which the cargo is found will be recorded by measuring the distance to the two nearest deck supports. The distances and numbers of supports will be recorded. Material will be recorded by the case that it is found in and not individually, except in instances where a case has rotted away. Cross-sections will be drawn of the ship for every reference point, and cargo will be drawn in place as it is recovered. The positions of cargo will also be recorded in relation to the distance to either side of the wreck. A study will be made relative to the condition of the cargo and the depth that it was buried in the sand. All ferreous objects will be stored in salt-water until ready for cleaning and preservation. The wreck will be photographed as often as possible to show the changes in it as it is salvaged.

Shipwrecks, Inc. will provide access to the wreck site for all interested archeologists who feel that they could contribute to the work. Papers will be published on all of the types of objects found. All offers of assistance in this line will be welcomed.

CARGO MANIFEST, 1ST VOYAGE

Shipped, in good order and well conditioned, by _____ in and upon the good Ship called the Mary Bowers Where of is Master of this present voyage Jesse De Horsey now riding at Anchor in the Port of Bermuda and bound for Nassau, N.P. being marked and numbered as in the margin, and are to be delivered in the like good Order and well conditioned at the aforesaid Port of Discharge. (the Act of God, the Queen's Enemies, Fire, & all & every other Dangers and Accidents of the Seas, Rivers, and Navigation, of whatever nature & kind soever excepted.) unto _____ or to _____ Assigns, Freight for the said Goods to be paid as for agreement primage and average accustomed. In Witness whereof the Master or Purser of the said Ship hath affirmed to _____ Bills of Lading, all of this tenor and Date, the one of which Bills being accomplished the others to stand void. Dated in St. Georges, Bermuda, July 26, 1864, Jesse De Horsey.

33 cases merchandise, 25 cases merchandise, 13 cases merchandise, 5 cases merchandise, 12 cases merchandise, 7 casks merchandise, 15 cases twine 3 packages.



Photo by G. H. S.

"HOPES FOR FINDING A LARGELY UNDAMAGED CARGO SOARED."

Figure 1

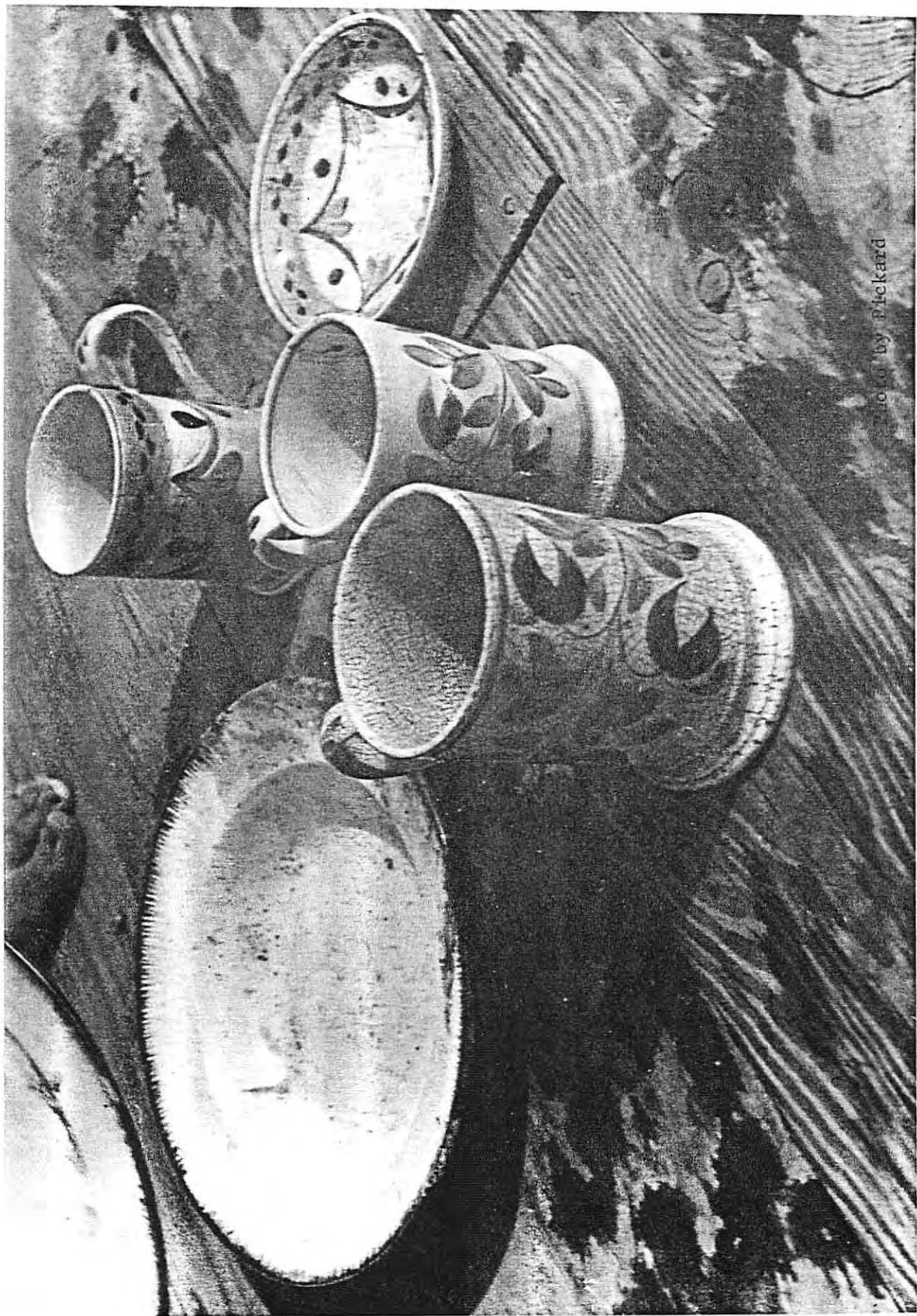


Photo by Pickard

"CHINA AS RECOVERED (UNCLEANED)"

Figure 2

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ANALYSES OF RESIDUE FROM CONTAINER FOUND
ON THE MARY BOWERS

Both chemical and microscopic analyses were made. Neither was an ultimate analysis.

Qualitative Chemical Analyses

Positive tests were obtained for:

Fe - Iron
Na - Sodium
Ca - Calcium
S - Sulfur

When a sample was treated with dilute hydrochloric (HCl) a considerable effervescence took place. Tests showed large quantities of H_2S and some CO_2 . Thus it is concluded that considerable organic material is present.

Semi Quantitative Chemical Analyses

Approximately 37% of the residue taken from the Mary Bowers is soluble in 6N - HCl.

Data - 1.0103 grams (sample)
 .6324 residue after HCl treatment
 .3779 soluble portion 37%

Another portion of the sample was "calcined" by extensive heating. The "ash" remaining was 36% of the original. This demonstrates that volatile, combustible and oxidizable materials are present.

Data - 1.1114 grams (sample)
 .4035 "ash"
 .7079

The "ash" was gray in color, compared to a dark original color. Thus the presence of carbon or carbonaceous material is indicated.

Moreover, the "ash" had a slightly reddish color, perhaps from iron oxide.

Conclusion from cursory chemical tests, the sample from the jar contains:

Iron
Calcium (perhaps as a carbonate)
Sulfur (as sulfides from organic origin)
Carbon (from carbonaceous materials)
Sodium

37% is soluble in dilute HCl.
63% is volatile, combustible or oxidizable.

Microscopic Analyses

The material for analysis was of two types. A high organic material, dark in color, and a very hard material cemented in part by the high organic paste. The sample was divided into (a) the high organic, and (b) the cemented material.

Data:

- (a) Sample was in the bottom of the jar.
- (b) Sample was on top of sample (a) and in contact with the ocean waters.

The samples were observed under 50X to 1250X magnification.

The following classes of material were found.

Large fragments or particles:

- (1) seaweed and annelid worm tubes
- (2) coral axial filaments
- (3) sand grains - 1 to 5 mm

Small fragments:

- (1) mica
- (2) small sand grains
- (3) iron
- (4) chitin

The large sand grains were cemented together by the organic paste which was composed of a mixture of the small fragments and the large fragments with the exception of the large sand grains.

Discussion of the Microscopic Analysis

Two processes have to be considered as having occurred either independently or concurrent to give the above data.

- I. The original material (if any) was consumed by bacteria, crabs, etc. very quickly and the jar filled with sediment.
- II. The jar was quickly capped by sand and mud and the following scene of events occurred:

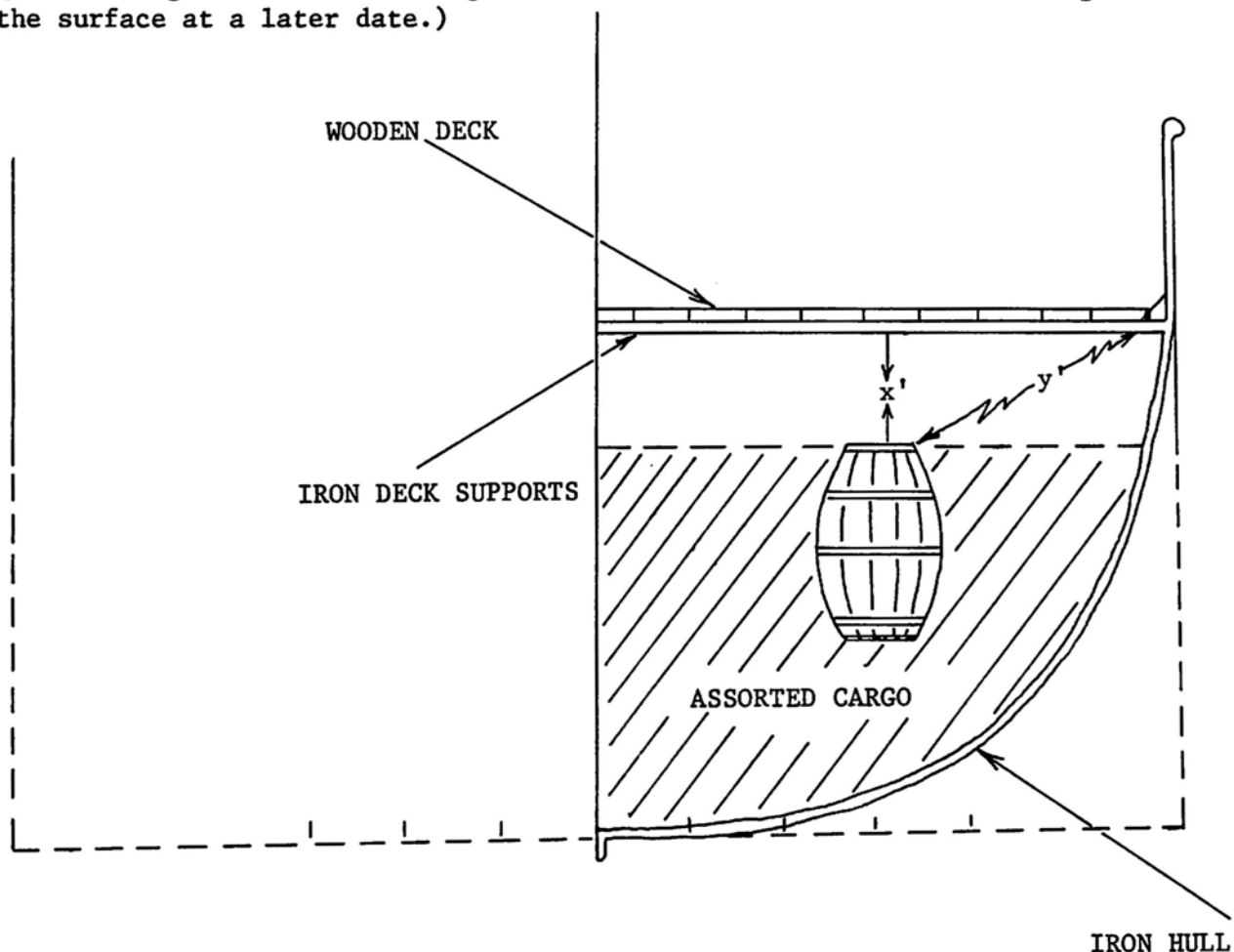
The available oxygen would have been completely exhausted in short order, leaving the high organic material. Anaerobic bacteria would have then consumed the oxygen bound to any sulfates, leaving hydrogen sulfide which combines with heavy metals (particularly iron) to form different sulfides. The result is a very dark color.

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The qualitative chemical analyses conducted at the Baptist College of Charleston, at Charleston, South Carolina, were conducted under the direction of Dr. William L. Hyden, Chairman, Division of Natural Sciences.

The microscopic analyses, also conducted at the Baptist College of Charleston, were conducted by Mr. James M. Dorn, Jr., Assistant Professor of Biology.

Sample diagram showing position of some major item such as a barrel or a keg. (A small keg of assorted sewing items has been found and will be brought to the surface at a later date.)



"The level that the cargo is found will be recorded by measuring to the two nearest deck supports. *** The positions of cargo will also be recorded in relation to the distance to either side of the wreck. Cross-sections will be drawn of the ship for every reference point and cargo will be drawn in place as it is recovered. A study will be made relative to the condition of the cargo and the depth that it was buried in the sand."

Further suggestions on how to derive the greatest possible amount of accurate information from this work will be given every consideration and credit due will be given in all instances.

A COMMENT ON THE RELATIONSHIP BETWEEN THE STATE
AND SALVAGE DIVING OPERATIONS*

Stanley South

Since the salvage license agreement with Shipwrecks, Inc. was the first issued by the State of South Carolina, it becomes a pilot case for future licenses. The Institute of Archeology and Anthropology is responsible for the awarding of such licenses and for making the division of the objects recovered in these salvage operations. Due to this involvement with the diving operation on the Mary Bowers it is appropriate that a comment be made on the achievements and disappointments of this adventure as outlined in Lee Spence's paper.

Lee and his colleagues are certainly to be commended for their positive attitude and leadership in working toward the passage of controlling legislation involving diving operations in South Carolina. However, Lee states that a "substantial" amount of the artifacts recovered will be retained by the State, whereas only one-fourth is so retained for study and exhibit purposes, and of this amount an effort is made to favor the diver in that broken or damaged items are often just as good for study purposes as the whole ones, and it is these that usually become a part of the State's share. Lee's position that "The State basically wants to achieve the same benefit from the salvage of these wrecks as it would if it was sponsoring the entire operation with its own funds," is not supported by the facts. If the State were sponsoring a diving operation with its own funds, strict requirements as to recording and recovery, preservation and study of the objects would be enforced, whereas with salvage diving, proper recovery techniques can be urged but not required, and thus much less information of value is forthcoming from such salvage operations than would result from a State sponsored underwater archaeological operation. This point is clearly seen in contrasting the admirable plans outlined by Lee before his major operation began, with the actual execution of these goals during the salvage operation. Since such a contrast is so familiar as to constitute a pattern wherever diving operations are undertaken, details of this contrast are presented here in the hope that future diving operations undertaken by private salvagers under permits issued by agencies of various states, can evolve toward more competently recorded salvage operations. When such becomes the case, then and only then can salvage operations take on the name of "archaeological salvage", for the use of the word "archaeological" implies a systematic approach to data recovery which is characteristically absent from salvage operations now being carried on in the United States. The exceptions are projects of the Smithsonian Institution, University of Pennsylvania, and the Canadian Historic Sites Service, which can legitimately be called "archaeological".

*This paper, presented at the Tenth Annual Conference on Historic Site Archaeology, is published here because of its relevance to Lee Spence's paper just preceeding.

Figure 1
(See Cover)

Polychrome Painted Pearlware Mug from the State of South Carolina's Collection of Artifacts from the Wreck Thought to Be that of the Mary Bowers. Cases of handleless cups and saucers and mugs in four sizes were being shipped to the Confederate States on English Blockade Runners such as the Mary Bowers. This type is now known to date as late as the 1860's, which may appear at first to be surprising. The possibility is seen that perhaps the English were cleaning out out-of-date stock from warehouses to be shipped to the Confederacy, thus accounting for the presence of this type ware at a somewhat later date than generally assigned to the type.

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In a salvage operation carried out by private diving companies on a historic wreck, certain basic questions are of interest from the State's point of view, and it is toward the answering of these questions that the State should orient its contract agreements with salvagers so as to produce the desired results. The divers will be primarily interested in artifact recovery, not in the process of recording data to answer questions asked by State representatives. The salvage companies are motivated by financial gain, as Lee has so well pointed out, and all else takes a second place to this motive in salvage operations. It is up to the states through their representative agencies and individuals to work toward the goal of making salvage operations more properly called archaeological salvage operations, and to deserve the title. The importance of this viewpoint cannot be over-emphasized, for some states have made little effort to require of the salvagers that proper records be kept of the process of data recovery. If the pattern of salvage diving to recover artifacts for monetary gain is not re-directed toward a systematic approach to wreck investigation in the decades to come, that systematic approach may always remain the exception. It is up to the states through their responsible agencies to not only issue diving licenses and permits to insure that the State's share of the artifacts is forthcoming from the effort, but to work through attitudes, requirements, and laws toward the achievement of a systematic approach to the investigation of the unique time capsules presented by shipwrecks.

In the case of Shipwrecks, Inc. the questions of interest could be answered by carrying out the following steps:

1. Drawings should be made and measurements taken of the visible wreck.
2. Photographs should be taken of areas of the wreck, or all of it if possible.
3. Drawings should be made of cargo relationships, where various objects come from.
4. Drawings and photographs of packaging of cargo should be made, below and/or above water.
5. Photographs of cargo immediately after being brought aboard should be systematically taken and records made at that time as to provenience.
6. A written report of visual observations should be made daily by the divers as to what they saw, accompanied by sketches whenever possible.
7. A systematic data recording system to catalog the objects and record their provenience should be carried out throughout the recovery process.
8. Objects should be kept under salt water until a division is made by representatives of the state agency involved. This can be done in cheap plastic pools, plastic lined boxes or pits, etc.

The above outline does not appear to be asking too much of the salvaging companies, yet it is more difficult to obtain this type of data from salvage operations. In his paper Lee discusses the proposed use of nylon cord markers, and the utilization of timbers within the vessel to establish provenience control on the cargo. He says he plans to record the recovery of objects as

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to depth, and to photograph underwater and above throughout the diving operation. All of these goals are admirable, but unfortunately almost none of them have been achieved. For instance, after a season of diving, we still do not have a drawing of the orientation of the wreck; there is no photograph of the appearance of the wreck on the bottom; there is no drawing of the relationship of the two wrecks to each other or to the shore; there is no verbal description from a diver; there is almost no information on how the cargo was packed; where it was located on the vessel; nor where it came from in terms of horizontal or vertical location. We do have the objects brought up, but few photographs have been taken, and no systematic control has been carried out. Thus, the artifacts recently divided between the State of South Carolina and Shipwrecks, Inc. are known to have come from somewhere on a wreck thought to be either the Mary Bowers or the Georgiana or both. These include a large quantity of rubber coated canvas sheets, porcelain buttons, sets of teeth for making dental plates, sticks of vermillion, a stoneware jug with an intact cork, large quantities of wooden spools with thread still intact on some examples, hundreds of blue-edged plates, polychrome painted cups, saucers, and mugs, Wedgwood bowls, wooden pencils with the name of the maker still visible, buckets of straight brass pins, wooden handles for tools, staff pens, and the contents of a fire making kit. All these objects are of considerable historical interest and will surely add to our understanding of specific objects being imported to the Confederate States during the Civil War. However, one still regrets the lack of control on such salvage projects.

This is not stated in a way of criticism of Shipwrecks, Inc., for they have been very cooperative in terms of attitude, and they also have to face practical realities of weather, strong currents, dangerous wreck conditions, availability of vessels and personnel, funds, etc. Their failure to fulfill their objectives as outlined in Lee's paper is pointed out here not to emphasize this fact, but to illustrate that this case represents a typical situation where a willingness to try to approach the data systematically is present, but the actual performance cannot reach these goals. Although the ideal goals were not accomplished here, the Institute of Archeology and Anthropology, archaeologists feel that definite and positive progress has been made toward a good working relationship between private divers and the Institute representing the State.

The need is for a more positive means of implementing these relatively simple data control procedures in shipwreck salvage operations by private salvagers. Although several states now have established procedures whereby salvage diving is carried out, the realities of salvage goals, to my knowledge, have always prevailed over those of data recovery. Therefore, the interests of the states in such operations have always come out second place to the goals and interests of the divers, regardless of how much their goals have been oriented toward data recording. Because of this situation, individuals and agencies representing the states are becoming accustomed to the recovery of artifacts from shipwrecks without the accompanying provenience drawings, photographs, observations, etc., characteristic of the process that is archaeology. The ironic thing about many of these operations is that they are called by their sponsors "underwater archaeology" rather than salvage operations. The stage of development at which the underwater recovery of artifacts from shipwrecks is

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at present equates with that period in our history when museums were concerned primarily with relics, and expeditions were sponsored throughout the world to recover art objects and unbroken artifacts for their exhibit value alone. Surely we do not have to go through the same painfully wasteful process in our exploration and recovery of information from sunken time capsules; yet, in these infant days of the exploration of the historical and archaeological treasures lying on the bottom of the seas, we are headed down that very same pathway!

In the field of archaeology there will likely always be those practitioners of the process who recover only a pitiful fraction of the data a site has to reveal, and whose efforts when compared with the more proficient craftsmen border on the criminal. So it will likely be in the recently expanding process of underwater exploration of shipwrecks and other underwater sites. The appeal here is for those representatives of the various states whose responsibility it is to represent the people in licenses with salvagers to make an effort to educate them toward becoming partners with the State in a remarkable challenge, the systematic recovery of information. Admittedly, this is a difficult challenge, but it cannot be met through strict legislation outlawing destructive practices unless it is accompanied by funds providing for an agency to enforce such a strict law. Since no such laws are at present in existence,*and since no regulatory forces are budgeted in most states, the effort must be made through cooperative action between the representatives of the state and the salvage companies. It is through this cooperative enterprise that the greatest progress toward systematic search and recovery of shipwrecks can be made. Under this arrangement, which is in effect in several states at present, a great deal depends upon the integrity and sense of public responsibility provided by the salvage divers. Under present laws in most states no attitude is realistic other than one of cooperation. The solution to many of the present problems will come when the states cease to depend upon the salvage divers to recover scientific and historical data and begin placing state personnel aboard diving vessels to insure that data is systematically collected. Laws authorizing the position of State Underwater Archaeologist with staff and operating budgets should outline the responsibilities existing between the State and the salvagers regarding systematic data recording, and provide for the establishment of a survey of underwater historic sites and wrecks. This state representative would draw up the contracts for diving operations and would take action where unauthorized or irresponsible salvage operations were being undertaken. Until underwater archaeological programs are created in the states involved, unfortunate looting of underwater sites will continue, both on the level of the weekend diver looking for bottles to full-scale operations by large salvage companies. Under the present system, state agencies dealing with salvagers will continue to have to rely on the integrity of the salvagers and on the amount of time the salvagers are willing to put into orderly recovery of data. Unfortunately not all salvagers are motivated as positively as Shipwrecks, Inc., and some flagrantly refuse to recognize any responsibility for data preservation. For such companies and individuals stringent laws and state financed personnel and budgets for the supervision of underwater salvage operations are the only answer. Such laws will gradually come into being in the years to come. Meanwhile, archaeologists, state representatives and conscientious salvagers can work toward bringing such legislation into effect in

* Texas has recently enacted such a law backed by state funds.

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a cooperative effort in the exciting challenge of discovery, recording, and interpretation of the historical story that lies in the streams and rivers, and beneath the waters of the sea. Until legislation is enacted to bring to salvage diving operations systematic data collection through state involvement in underwater archaeology programs, the efforts at recovery of historical and scientific data from underwater sites will continue to remain in the realm of salvage relic hunting, and does not deserve to be referred to as "underwater archaeology".

SUMMARY OF SPECIFIC NEEDS FOR A STATE SPONSORED UNDERWATER ARCHAEOLOGY PROGRAM

An underwater archaeologist is needed to supervise contract agreements with salvagers, conduct a state-wide underwater site survey, and to be the state representative on-board diving vessels to supervise the following:

1. A diver to take underwater photographs, make sketches, drawings, and to take verbal notes.
2. Photography of artifacts as they are brought on deck.
3. Cataloging of items and provenience recording.
4. Supervise immediate preservation steps for some items.

These tasks can be fulfilled by the archaeologist and a staff of two, all three of which should be divers of experience and competence.

In order to properly carry out an underwater archaeological program with sufficient data control for it to be called "archaeological", these state personnel should be provided for in any new legislation aimed at recovery of data from underwater sites. In addition to these functions relative to specific diving operations, the personnel of state underwater archaeology programs would be responsible for the systematic location of historic wrecks and sites through survey in documentary as well as on site locations. The data from such a survey would become a major source of information which would allow for the evaluation of the historic wrecks and sites. Through such a classification, projects could be undertaken on the basis of the potential value a wreck would yield in terms of historical and other criteria. Permits for divers interested in search and survey of wrecks could thus be assigned, as well as salvage diving operation permits, on the basis of the classification of the wreck site. Under such a program, some wrecks might well be declared off limits to diving of any kind. Again, the significant point this paper is attempting to make is that any such program should be financed by the state governments involved, or by the Federal government. The time has come for governments to stop relying on private sponsors to recover scientific data from underwater sites.

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A final word should be said regarding the needs as outlined here relative to underwater archaeology programs. This outline is specifically oriented toward a minimum program and not to a major archaeological project involving an underwater grid system complete with recording, such as is carried out by George Bass through the Smithsonian sponsorship. It is felt that in many cases such underwater work will not be possible due to problems of visibility, currents, etc., but in such instances, systematic recording and recovery can still be effected, provided there is sufficient legislation to protect the wreck sites and provide for at least a minimum program of systematic underwater archaeology as outlined in this paper.

As was mentioned earlier, several states are in the process of enacting laws designed to establish the relationship between the state and salvage divers operating on historic wrecks and sites. It is important that these laws be made strong enough to protect the sites while at the same time not being so prohibitive as to be unenforcable, and therefore virtually useless. Any legislation aimed at protection of wreck sites should be provided with the teeth of government funds to be most effective and realistic.

A COMMENT ON DIVING OPERATIONS
IN SOUTH CAROLINA

Lee Spence

Recently I received a letter from Mr. Stanley South, whom I have known for several years, and whom I have always considered a great archeologist and a friend. Unfortunately, Stan is like so many persons connected with historical archeology (including myself), he is so deeply involved in his own work that he is unable to completely understand the problems or work of others. He is so interested in the success of all archeological projects, that he is often disappointed with what is actually a major achievement, solely because his dreams for the project were too high to have ever been satisfied, or even approached. Stan's letter carried a copy of his paper "A Comment on the Relationship Between the State and Salvage Diving Operations". He asked me to read it and to write a reply to it. Stan's paper outlined the achievements and disappointments in the salvage project conducted on the wreck identified by myself as the Civil War blockade-runner, Mary Bowers, sunk in late 1864, off of the Isle of Palms, near Charleston, South Carolina.

In his paper Stan points out the great plans and hopes for the operation that were described in my paper "Salvaging the Cargo of the Mary Bowers", which I presented before the Historic Site Archaeology Conference in November of 1968. I am writing the following as a part rebuttal of his statements, part agreement with his statements, and part challenge to the State to go even farther along the lines that Stan has suggested. I commend Stan for the position that he has taken on the salvage work as it is the only reasonable position that could be taken by someone trying to protect the State's interests. I would like to point out that I am no longer associated in any way with Shipwrecks, Inc. and that I was not in a policy making position with Shipwrecks, Inc. when the salvage work was performed.* I would also like to point out that I was opposed to a great deal of what was done on the wreck, as I felt that valuable information was being lost, but I also realize the position that the State put Shipwrecks, Inc. in and it is mainly for that reason that I am taking the time to write this reply to Stan's paper. I also realize that none of this, whether good or bad, would have taken place without my instigation.

I thank Stan for his commendation of my efforts to secure proper legislation and State guidance for salvage work in South Carolina waters. In this he is in effect acknowledging that the State needed some rules and regulations and that although professional archeologists in the State had either never tried, or had never succeeded in passing such legislation. A group of concerned amateur archeologists led by myself did succeed.

* I sold my interest in Shipwrecks, Inc. because I disagreed with the policies that were being followed.

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Immediately after Stan's commendation of my efforts the axe begins to fall. Stan says, "Lee states that a 'substantial' amount of the artifacts recovered will be retained by the State, whereas only one-fourth is so retained for study and exhibit purposes, and of this amount an effort is made to favor the diver in that broken or damaged items are often just as good for study purposes as the whole ones, and it is these that usually become a part of the State's share." Since an archeologist is generally interested only in what can be learned from the artifacts and not in their immediate sale value, I feel that one-fourth of the artifacts recovered is indeed a substantial amount. (Out of the remaining artifacts the salvage company must pay for rental or wear and tear on equipment, pay the divers, repay any loans made to finance the operation, put some money aside for future operations, and hopefully still have some artifacts left for their own personal use, such as setting up a museum or donating a display to one of the many public institutions who have supported the work in one form or another. The Baptist College at Charleston is a typical example in that it loaned Shipwrecks, Inc. the scientific equipment needed to analyze some of the materials recovered. Some money and some of the artifacts must be set aside by the company to conduct experiments on the preservation of the artifacts.) Stan is correct in that the State does go out of its way to accept broken or damaged items. Since Stan represented the State in the original split, I had assumed that he felt that a fair split was made. There was a great deal of bargaining that took place at the split. Stan first divided up all of the items taking one-fourth of all of the whole items and one-fourth of all of the broken items. Stan then proceeded to trade some of the perfect items for large amounts of broken items which he felt could be restored and studied. By doing this, Stan was able to greatly increase the State's cut, percentage wise, raising the State's take far over the original one-fourth agreed on. This was agreeable to Shipwrecks, Inc. because although it meant fewer artifacts, it also meant more perfect items that could be easily converted to badly needed cash.

Stan then says, "Lee's position that 'the State basically wants to achieve the same benefit from the salvage of these wrecks as it would if it was sponsoring the entire operation with its own funds', is not supported by facts". If Stan is correct, then the State should be condemned for its position. I feel that the State should want exactly what I said. If it does not demand this from a salvage company, then the State is failing its citizens. I completely agree with Stan's contention that the State did not receive the same benefit, but I do feel that with proper guidance, Shipwrecks, Inc. was capable of giving it.

According to Stan, if the "State were sponsoring a diving operation with its own funds, strict requirements as to recording and recovery, preservation, and study of the objects would be enforced, whereas with salvage diving, property recovery can be urged but not required." I do not think that the salvage company's wants or desires could or should take priority over the State's jurisdiction. Perhaps the State could

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require it, but it might be unrealistic to do so, but some compromise could be worked out. Stan says that "systematic approach to data recovery" is characteristically absent from salvage operations now being carried on in the United States. In this Stan is correct, and I agree that the salvage work on the Mary Bowers was a classic example. This is unfortunate but directly due to the lack of State guidance, I feel, during the operation. Shipwrecks, Inc. is a regular corporation in that it issues stock and works under a State granted charter. The company must be run according to the will of the majority of the stockholders, providing of course that the company operates within the framework of the State laws governing its type of work. As Stan has so well pointed out, this type of operation leaves no room for adding on expensive record keeping and slowdown of work solely out of a feeling of public duty. If something is an added expense, and is not required by the State, a company is certainly going to avoid doing it, to keep the expenses down. It is the company's obligation to its stockholders, just as it is the State's obligation to see that proper guidelines are set up and required. For the State to purchase the equipment that Shipwrecks, Inc. had at its disposal, the State would have to invest over a quarter of a million dollars. It is understandable that they worked in the manner that kept the cash outlay to a minimum, but the State's part is neither understandable or acceptable. The State should have threatened to cut off Shipwrecks, Inc.'s permit if it felt that Shipwrecks, Inc. was doing an improper job. I am positive that a little forcefulness on the State's part would have gone a long way in solving the problems. I would like to go ahead and point out that less than 5% of the total work to be done on the wreck has been done to date, and it is far from too late to make the needed changes. I would also like to add that although I was the person that identified these wrecks, I could be mistaken, and I cannot be sure of my identification until further work has been done.

Shipwrecks, Inc. should be commended for its leadership and efforts. Shipwrecks, Inc. even went so far as to negotiate a contract with Real Eight Co., Inc. of Florida and have them bring their salvage boat and equipment up from Florida with their professional diving team, which included Robert F. Marx who directed the salvage of the sunken city of Port Royal, Jamaica. Real Eight Co., Inc. invested a lot of time and a great deal of money in the project and due to weather conditions, such as wind, rain, and snow, Real Eight Co., Inc. was able to make only a few dives on the site. In spite of the extremely dangerous diving conditions and the limited amount of time actually spent in the water, a considerable number of artifacts were recovered, but certainly by no means enough to pay for the expense of the work. (Remember, only people hoping to gain some financial return would ever have dived on the wreck and even then only people as capable as Real Eight's divers and as interested in their work as Real Eight's divers would ever have gone ahead and accomplished something - as they did - while they were on the bottom.) In actuality, Real Eight never received any compensation for the work that they did and they were never able to keep even one artifact that they recovered. The State delayed so long in splitting up the artifacts that Real Eight Co., Inc. had long before returned to Florida

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with nothing but expenses to show for their efforts. Real Eight Co., Inc. is, in my opinion, the finest salvage company in America, and a large part of my opinion is based on my personal knowledge of their high sense of integrity. For a treasure company, they have some of the most modern equipment and finest staff in existence.

Stan says, "some states have made little effort to require of the salvagers that proper records be kept of the process of data recovery." Here Stan may be speaking of South Carolina, as this state made almost no attempt to direct the proper keeping of records. The original records kept of the first dives made on the wrecks were made by myself, months before there was any law in South Carolina specifically protecting the State's interests. When the law was passed, it placed the responsibility of directing the diving into the hands of the State Archives & History Department. My records were examined and okayed by Mr. Barney Slawson, who was then in the position of representing the State. He said that in the future, the records should be kept the same way. Unfortunately, when the direction of the diving shifted to the Institute of Archeology and Anthropology at the University of South Carolina, the only comment about the records that I had kept was the statement that they sounded like something out of an adventure magazine. No attempt was made to show me how to write down the records as cold dry statistics or how I should keep them to satisfy the Institute.

I am not an archeologist in that I have no formal education in archeology, but I have been doing private research on shipwrecks and reading about marine archeology for over seven years. Others are more qualified to interpret the significance of the artifacts that I recover, but I am certainly qualified to speak of the problems involved in diving on a modern shipwreck. My reports, although seemingly adventurous, were written up in recognition of the fact that few people have ever done any extensive diving on an iron wreck that has had a period of one hundred years to deteriorate and transform from a ship into a trap over 200 feet long. In my reports, I tried to describe what it was like to dive on a wreck with visibility ranging from absolute zero with total darkness to just over thirty feet, and getting tangled in shrimp nets and loose line. I felt and still feel that it is important to point out how easy it is to dislodge the loose iron plating on the wreck and to tell of the dangers of getting cut on jagged iron barnacles. How many people would stop to think that safety lines can be a danger to the diver in water with zero visibility and so cold that the divers can't even feel the line that may entrap them. A natural thing is to tie off the salvage boat directly to the wreck so that the diver won't have to use up precious energy swimming back and forth to the boat. But this procedure almost cost me my life once when a large swell lifted the boat up (a 72 foot shrimp trawler) and pulled the large piece of iron free that it was tied off to (we were using heavy stainless steel cable as an anchor line). The boat then swung the piece of iron wreckage over me and set it back down, crushing me into the mud. Fortunately, another swell picked it back up again and enabled me to swim, or rather crawl, out from under it. Is there a place in a regular

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archeologist's report to record such an incident? Yet it should be recorded, so that others will not repeat the mistake. I could list numerous similar experiences and whether they are adventurous or not I can't really say, but the recording of them is important.

Stan next gives a list of 8 procedures that should be followed on all diving operations. Drawings were made of parts of the wreck and some measurements were taken during the initial work. These were compared with the plans of the Mary Bowers and she was identified largely by her hull construction. The wreck was so extensive that an overall picture of the wreck was virtually impossible to come up with. Due to such hazards as old shrimp nets and overhanging iron, it is impossible to swim a straight line from one end of the wreck to the other. Several early sketches of individual portions of the wreckage were made and shown to representatives of the State and all later drawings were made for the sole use of the divers involved and no attempt was made to preserve them for the State, as the State had set no policy requiring them. We did assume that the State wanted a final overall drawing of the wreck but this was never achieved because of the problems outlined above. (I personally feel that all salvage companies should be required to map the entire area before any salvage work is performed.) Photography was next to impossible on the wreck due to the limited visibility. Pictures were taken of the wreck, but they were taken for the personal use of Shipwrecks, Inc. and to my knowledge the State has never made any kind of official request to see them. I am sure that Shipwrecks, Inc. would be happy to provide the State with copies of the few photographs that were taken if the State would start making its wishes known to the persons involved. (Remember, I am not an owner or member of Shipwrecks, Inc., so although I know that the State wants to see the photographs, I can't do anything about it.) Stan suggests that the salvage companies be required to store the artifacts in salt water until they can be properly treated and preserved. I agree, it is a very good idea. I made the same suggestion to Stan almost two years ago and repeated the suggestion in a letter a few months later to Sam Townsend of the North Carolina Department of Archives and History. I must admit that after the running of the salvage operation was out of my hands, that salt water was no longer used to store the artifacts. Again, I repeat the same stock reason for this - it was an added expense that was not required by the State or even officially requested.

Stan is not alone in his regret that the State achieved so few of the high hopes that I outlined in my paper, but I had wrongly assumed that the State would back me up in my plans, making my suggestions the official desires of the State. My ideas are not necessarily original or the final answer to the State's problems, but they are ideas which could be followed. South Carolina should adopt some of the policies of Florida and North Carolina which both seem to be doing a fine job of directing the salvage work under their jurisdiction.

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Several times I have pointed out cases of looting of underwater sites in South Carolina waters, giving the names of places involved and how it could be prevented, but Stan's answer was that the State has no funds for policing or development of underwater sites. My feelings over this were really aroused when I applied in the name of Artifacts, Inc. (a company that I own an interest in) for the salvage rights to the area around Childsbury on the Cooper River above Charleston, S. C. The State made no acknowledgement of the application other than a verbal promise to grant the salvage rights at a later date (this has never been done). In the meantime, divers looted the site to such an extent that finally a verbal okay was given to a number of groups to look for artifacts and report what was found. I feel that the State should act promptly on all applications for salvage permits and that nothing should be left out in correspondence concerning the permits. All agreements and requests should be made in writing. The State should make its reasons for delaying or not granting a permit clear, and should impose any restrictions on the salvage company that it wishes, but should put these special restrictions in writing at the time the original permit is granted.

I would like to suggest that for each salvage diving project a file should be kept regarding all correspondence between the State and the divers, along with the official contracts. I think that a complete inventory of all of the items recovered should be included, and if possible, pictures of each type of artifact should be shown. I also feel that a similar report should be prepared each year although I don't feel that it should necessarily be published and widely distributed. Many people may feel that I am asking for privileged information, but rightfully it is public property and there should be some condensation or publication of diving efforts for public use. By doing this, the public will have the opportunity to learn what is going on, and possibly add some constructive criticism.*

I hope this reply has not been offensive, but if I did not respect Stan and his integrity, I would not have written a reply such as this. I feel that we owe Stan a vote of thanks for providing a forum for the expression of ideas on this subject.

*[Editor's Note]

Such files are kept at The Institute of Archeology and Anthropology. Lee's idea for a publication outlet for reporting on diving efforts is a good one. At present, The Archeological Society of South Carolina sponsors a Notebook, but this has not yet dealt with diving operations. A publication outlet such as a diver's Notebook might be a good undertaking for the recently organized South Carolina Underwater Research Council, which is co-chaired by Mr. Roland C. Young of Columbia and by Lee.

REACTIONS TO THE TWO PAPERS

"SALVAGING THE CARGO OF THE MARY BOWERS"

BY LEE SPENCE

AND "A COMMENT ON THE RELATIONSHIP

BETWEEN THE STATE AND SALVAGE DIVING OPERATIONS"

BY STANLEY SOUTH

Robert C. Wheeler

Although the underwater program in Minnesota is not faced with quite the same problems one encounters in coastal areas, I found the two papers enlightening and relevant. First, may I make it clear that I am grateful for all the contributions made by so called "amateur archaeologists" and divers, and I am not in favor of stamping them out, as some archaeologists say they would. As in land archaeology, the majority of underwater archaeological finds have been made by amateurs, a fact which the professionals should not forget. At the same time, I am sympathetic with the point of view taken by the archaeologist. At a number of meetings on underwater archaeology, we have discussed this very subject -- whether or not the professionals can or should cooperate with the amateurs. Inevitably the discussions last for hours and end in a stalemate.

I get the impression from the Spence paper that here is a well-meaning man who sincerely wants to make a contribution to archaeology, and yet he finds himself in a dilemma. In the first place, he is not an archaeologist, and secondly, in the salvage business, he finds himself facing a multiplicity of problems -- paying for equipment and divers and beating off competitors. It is easy to understand how well-meaning individuals can make promises on the surface to follow archaeological guidelines and to fulfill the requirements set forth in salvage licenses. However, on the bottom, faced with heavy current, lack of visibility, extreme temperatures, and other problems, one can soon forget promises and obligations. I can appreciate the position of the salvor, but this doesn't solve the problem either for the salvor or the archaeologist.

Now a few comments on the paper by Stanley South. South represents the professional point of view, that of the archaeologist. I am pleased to see that South commends Lee for his positive attitude, leadership, and his willingness to work with the Institute of Archeology and Anthropology, however, South quickly challenges Lee's position -- that "the State basically wants to achieve the same benefit from the salvage of these wrecks as it would if it was sponsoring the entire operation with its own funds." South calls this statement "not supported by facts." He continues "if the State were sponsoring a diving operation with its own funds, strict requirements as to recording and recovery, preservation and study of the objects would be enforced, whereas with salvage diving proper recovery techniques can be urged but not required, and thus much less information of value is

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forthcoming from such salvage operations than would result from a State-sponsored underwater archaeological operation." South calls Lee's plans "admirable," but indicates that in actual execution, the accomplishments were considerably less. Obviously the results of this marriage of salvors with the Institute of Archeology and Anthropology has not been a satisfactory one. At least this is the impression I get. South says "I still regret the lack of control on such salvage projects." Personally, I believe he comes closer to the mark when he writes "the solution of many of the present problems will come when the States cease to depend on the salvage divers to recover scientific and historical data and begin placing state personnel aboard diving vessels to insure that data is systematically collected." Readers will begin to realize how serious this problem has become when South, speaking of the Lee Spence operation, says "not all salvagers are motivated as positively as Shipwrecks, Incorporated." Certainly no single solution is going to solve all the problems. I believe it will take a combination of legislation, better cooperation and communication between divers and archaeologists, and a realization on the part of state officials that funds must be forthcoming to provide trained personnel, prohibitive legislation, and money to provide the necessary controls.

Recently I attended a meeting on underwater archaeology in Bethlehem, Pennsylvania. There were a number of states represented, including several states bordering the ocean where problems with salvors were being encountered. Even in a state where salvors are licensed and required to have supervision by an archaeologist, the arrangement wasn't proving satisfactory. Unquestionably the job of scientifically excavating all of the shipwrecks along the coasts is an impossibility, but perhaps certain wrecks, those to be found of great historical importance could be declared "off limits." These might be in category #1 and under no circumstance could these be excavated by salvage companies. Salvage companies could only work on those wrecks which have been placed in category #2 or #3. Category #2 would be those of historical interest requiring archaeological guidelines and a minimum of professional supervision. Those wrecks -- category #3 -- might be left wide open.

Not long ago I received a copy of a newsletter from a diver who had attended the Bethlehem meeting and was commenting upon his experience at the underwater session. I take the liberty of quoting from their group's newsletter, for I think the sooner we make some distinctions and begin working with the serious ones, who can make a contribution, the better. I quote "On the Thursday evening, a special session on underwater was held dealing with the "increasing destruction of underwater sites and what can be done." The panel consisted entirely of professional archaeologists and historians who are dealing with this problem. I personally was aware of the attitude of the professional's feelings to the amateur diver but was totally unprepared for the severe criticism that was made. In many cases, I was inclined to side with them. Unfortunatley, however, there was no distinction made between the serious, dedicated amateur and the treasure or pot hunter. It became very evident that all amateurs are considered unfit to be involved in any phase of the work unless under the direct supervision of a professional, of which there are sadly too few interested, especially in the Great Lakes. This attitude was completely unanimous among all present

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although several of the discussants made some defense of some serious amateurs. Being the only so called amateur in the room, I felt most uneasy and unwelcome even though I was aware of the serious efforts by many of our people in Ontario. I also felt that it would have been sheer suicide to have made any defense on our behalf. My feeling is that we must now increase our efforts in all phases of this field, become even more proficient, extend our co-operation to the interested professional, and attempt to show that we are worthy of some consideration in our efforts. One factor in our favor is, that in general, there are no treasure ships in the Great Lakes and we are not dealing with a similar situation as Florida. The "pothunters" in our area do not seem to be as enthusiastic in their efforts as we in ours.

Perhaps at the next meeting on the 28th of this month, some serious discussions on this problem can be held between the serious diver and the professionals in attendance. If the same feelings persist here as did in Bethlehem, then perhaps we have been wasting our valuable time, efforts, and great personal expense in a belief that we have something to offer the people of Ontario. Think about it!!

Quite recently I ran across an article in the September 28, 1969, London Observer. A few excerpts will, I think, reveal that similar problems are approaching solution in other countries, and that failure on the part of archaeologists and divers to cooperate can only lead to frustration, enmity, and an almost total lack of accomplishment. From the London Observer: "The really bitter conflicts arise when the historical value of a wreck is ruined by 'plundering'. ...In Britain finds on land come under the law of treasure-trove and go before coroners' juries. Underwater finds must be reported to the local Receiver of Wreck, a Board of Trade official. If after one year the treasure is unclaimed, it is sold to the highest bidder and the salvor receives a proportion of the proceeds (usually about one-third)." The matter is rarely as simple as this, however. Where more than one party has tackled a treasure site, it may be difficult to establish the salvor in possession. Legal precedents suggest that anyone finding treasure should leave guidelines and other evidence of their presence -- unfortunately, this can be an open invitation to looters.

The law is also criticised because the system of selling to the highest bidder often leads to the treasure going overseas or to those with no genuine archaeological interest. Museums can rarely afford to buy.

Archaeologists also complain that ancient finds under the sea are given no special protection, as on land. In Western Australia, certain wrecks (including some undiscovered ones) have been named as of archaeological importance. Most Mediterranean and Scandinavian countries apply antiquities laws to the sea, insisting that a wreck must be reported before it is investigated.

Archaeologists would like some changes to be made in British law along these lines, but are reluctant to see divers become, as they have become in some parts of the Mediterranean, objects of suspicion. Miss Joan du Plat Taylor, secretary of the Committee of Nautical Archaeology, which was established by representatives of diving clubs, archaeological institutes and

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museums in 1964, believes that treasure-hunters can be persuaded to co-operate with archaeologists. Their interests, she believes, are more convergent than is generally thought." ... "Lieutenant-Commander Alan Bax and Mr. Jim Gill, a Dorset architect, founded a School of Nautical Archaeology in Plymouth last February, with the backing of the Committee for Nautical Archaeology and of the British Sub-Aqua Club which, with 10,000 active members, is recognized as the ruling body for diving in this country. The school has been running weekend lecture programmes throughout the summer, along with practical work on two sites, each containing a number of cannon.

Commander Bax himself welcomes treasure-hunters to his school, as both students and lecturers. 'Goodwill is worth much more than laws, which cannot be enforced and are only made to be broken,' he said.

He proposes a system whereby Receivers of Wreck would automatically inform the Committee for Nautical Archaeology when finds are made, thus introducing some official recognition of the undersea archaeologist's needs. In return for the opportunity to study the wreck from the historical angle, the archaeologists could help identify and assess the value of the finds, advise on the salvage work, and, if necessary, organize voluntary helpers for the operation.

'If we could achieve a solution like this,' said Commander Bax, 'Britain might lead the world in underwater exploration. It would prevent the situation developing into a game of "cops and robbers". Instead we could encourage wreck exploration in an atmosphere of peaceful co-existence, each side respecting the needs and aims of the other.' Which way are we in America going to go?

I feel it would help if we would clarify one thing. In the first place, there should be no such term as "amateur archaeologist". One is either an archaeologist or he isn't an archaeologist. Is there an amateur doctor? The term "amateur archaeologist" raises the ire of the archaeologist and one can appreciate why.

It would also be very helpful if those vitally interested in the problem of underwater archaeology would establish an organization, the members, in addition to sharing knowledge, could influence public opinion and effect needed legislation. This national, or even international, organization of underwater archaeologists could be doing an important job throughout the world. Mr. South mentioned the need for the states to get together and to solve problems. Such an organization, with regular meetings, could do just that.

Other things need clarification. As Mr. South points out, many projects are termed "archaeology" when they are nothing more than salvage operations. The term "archaeology" is often used to give the diving activity an aura of respectability. Hopefully some day there will be more universities offering courses in underwater archaeology.

Minnesota has an antiquities law which includes underwater archaeology or rather underwater sites and materials of historical importance. The trouble here, as in most states, is that the law is difficult, if not impossi-

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ble, to enforce. There is one bright spot on the horizon, however, and that is our recent cooperation with the Minnesota Conservation Department. One hundred and fifty game wardens or conservation officers, as they are called, operate in every section of the state. They can act as the eyes and the ears for our program. If there is any infringement on historical areas or any vandalism at historical sites, both on land and underwater, they notify us. At the next session of the legislature, we intend to grant them legal authority to act in our behalf. Hopefully some day funds will be provided to put one or more archaeologists in the field to deal with the discipline of underwater archaeology.

SALVAGE VERSUS ARCHAEOLOGY

Peter Throckmorton

Some comments on the development of marine archaeology since World War II, and lessons to be learned from this in creating sensible legislation in South Carolina.

The first considerations are still basic: what is an archaeological object in the sea? What, for that matter, is marine archaeology? Attitudes toward the past change, from one time or place to another. A 17th century Pope tore down Constantine's basilica of St. Peter in Rome, and did it to glorify God through decorating Rome. Certainly he considered it no act of culpable destruction to replace an ancient building with a modern and "better" one, yet today we are appalled.

Naturally, objects that will eventually be of historical interest seem, at the time, to their contemporaries, an unlimited source quite possibly of doubtful interest. This is especially so in regard to ships. An obsolete ship on our industrial society is so many tons of scrap, seldom more.

Many of us who like sailing ships and are over forty years old now can remember walking the decks of some of the great sailing ships that created America and the rest of the modern world. Yet since we were children on those boats, we have seen the ships themselves abandoned to weather or scrap yards again and again. Little physical evidence remains of that long maritime tradition.

Ships like the Victory, the Cutty Sark, and the Joseph Conrad have indeed been saved, because of great effort on the part of a few far seeing, stubborn people. Even today the idea of preserving historical ships in organized maritime museums is relatively new.

We shall avoid defining precisely what makes an object "archaeological", the answer falling in individual cases somewhere between local laws and private sentiment - there is nothing like archaeology for demonstrating that one man's treasure is the next man's trash. Pragmatically, the point is that we are losing our physical heritage, partly through neglect, partly through intention, as modern technology demands more and more space and materials to satisfy its demands for growth. We let our virgin stands of timber be cut down and have not yet completed our recovery from this mistake. There is no second chance with the wrecks of old ships. There will never be any more.

Reasonable people still ask, why spend money, public or private, on preserving junk? Plans exist of the American sailing ships for those who are concerned. Why not spend that sort of money on libraries? The wreck

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of the Mary Bowers and her cargo, for example, or for that matter the Victory or the USS Constitution or even the Spirit of Saint Louis, are of little importance to professional historians.

Probably students and amateurs benefit most from museums. We all study history, and teenagers suffering through dry words with occasional pictures, are certainly helped, or even inspired, by seeing objects themselves. If students are to understand the last days of the Confederacy, it seems a shame to throw away such a magnificent teaching aid as the wreck of the Mary Bowers.

The state of South Carolina may or may not decide to make appropriate legislation about this wreck and others, or set up a section of marine archaeology for ships dating before 1865, or spend the money to save wrecks for posterity. The decision is not one of sentiment, but of finances.

Will it pay the State to spend perhaps a million dollars to preserve the Mary Bowers? If the wreck is salvaged and restored, how much will it cost, and how many years will it take before this investment is returned in terms of paid admissions to a museum? There are guidelines here. The San Francisco Maritime Association has restored a series of 19th century ships, and the San Francisco Maritime Museum actually makes a profit off paid admissions.

In his paper, Stanley South questions the state policy of turning over historical shipwrecks to commercial salvage companies, as well as the salvage companies' ability to carry out archaeology as opposed to salvage. I am in complete agreement, and would carry it still further and say that my experience in both fields convinces me that no commercial venture can carry out a correct excavation of an historical ship and show a reasonable profit. Nor has any commercial venture ever done so.

The only possible exception to this proposition is a case like that of the 1715 Treasure Fleet, and I should say that even here the question of profit is doubtful.

Having been at one time a commercial salvager myself, I speak from experience in saying that the only way to make a profit is to dynamite a wreck, collect whatever is of value, and get out fast. Time wasted is money lost.

In 1960 George Bass, Frederic Dumas and I worked together excavating the wreck of a Bronze Age ship off the Turkish coast. Dumas and I had been trained as commercial divers, and were shocked at the way Bass "wasted" time on the bottom. He, however, had been trained as a classical archaeologist. His contention, and ours, once we thought about it, was that an insight into how the cargo was stowed, how the Bronze Age sailors lived and worked, was what the whole project was about, and was worth a whole summer of diving hours even though it was costing over fifty dollars an hour and we were running out of money.

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Bass's idea was and is that nobody but an archaeologist should be in charge of an archaeological excavation. He knew the facts of what had gone on in southern France, where excavations had been done by professional seamen, divers and salvagers, with a strong uncommercial interest in archaeology. Archaeologists were brought along as poor relations on board ship and made fools of by the hearty divers from time to time. Though they were trained to record and understand finds from the past, they agreed with the divers that they had no competence to decide on possibilities of making drawings and photographs underwater, or decisions about methods of excavation and recovery. Their principle concern with ships, ancient or modern, was avoiding the disgrace of sea-sickness.

The result was that, in one notorious excavation, half a million dollars was spent, color photographs of the excavation were published around the world, but today, nearly twenty years later, there is still discussion and doubt as to whether they excavated one shipwreck or two.

George Bass doesn't claim to be a professional diver, or seaman, or engineer. His pride is in being a good working archaeologist. In the past ten years he has presided over more technical innovations than has any single person in the history of archaeology, including detection of wrecks with side scan sonar, undersea photogrammetry, and the first accurate drawings ever made of a deep water site. This is the whole point - the precise recording of objects so that we may interpret our own history.

Since the southern Turkey excavation, the University of Pennsylvania Museum Underwater Archaeology section has carried out major excavations of ancient ships in Turkey, in Cyprus, and in southern Italy. Other members of the group and I, working either through the museum, with other groups, or as individuals, have worked on everything from Roman seaports to preEtruscan lake dwellings to shiploads of silver coins in the Indian Ocean.

In no case has the value of what we recovered equalled, in a commercial sense, the expenditure involved in the excavation. The silver coin wreck is particularly relevant - the coins were indeed worth something, perhaps \$200 apiece for the best, when we found them; but once news got around of the existence of about a ton of them they were worth, at an optimistic estimate, maybe \$2 apiece.

But there are other ways of making profit. The village of Bodrum in southern Turkey, now center of the operations directed by George Bass in that area, was a poor and primitive place when I first arrived there in 1958. Now its crusader castle has been made into a museum, the central attraction of a steady tourist business, and most of the villagers are better off. The University Museum has a similar project, less developed, going on in southern Italy and another in Cyprus, all built around shipwreck excavations.

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Our projects have produced very little of intrinsic value, but it does seem clear that archaeological projects can be profitably "exploited", not only in terms of knowledge gained but of actual money. But the initial investment is too large, the returns too slow, to be of use to commercial companies. Private museums run for personal profit may benefit the individual profiteer for a short time, and often the antiquities themselves disappear. Only the state or a large university museum can afford to invest the kind of money needed, and only such an institution can profit by such an investment.

Various legal controls of the undersea antiquity situation have been tried, few of them successful. Here in the Mediterranean, where we have worked for the past twelve years, we have seen the development of many of these systems meant to preserve archaeological shipwrecks. There are many more wrecks here than in the United States, and the dateline dividing salvage from archaeology, though often put somewhere in the 19th century, extends much further into the past.

Nearly all the Mediterranean countries have serious antiquity problems, on land as well as in the sea. Unlike the United States, most of these countries have an archaeological service, often on a ministerial level, staffed by trained archaeologists and financed by the government.

Here antiquities belong to the state. Illegal exploitation is a bad problem. In districts where the local authority does its job well, there is a good deal of hope. In areas where archaeological appointments are governed by pork barrel politics, the situation is desperate. All the local authorities, good with bad, are hampered by the vast amount of policing and excavation which must be done with limited funds.

This situation should never exist in the United States, where antiquities are relatively rare, and where much more money exists for this kind of work.

The French have had among the worst of troubles with antiquities in the sea. Their southern coast was the first to be invaded by skin divers, beginning in 1947 when the Aqua-lung caught on there. Literally hundreds of shipwrecks were found. The government, navy, archaeologists, and a number of divers all did what they could to control the large scale looting which occurred, but by now nearly all the visible wrecks in depths shallower than 165 feet have been completely destroyed.

Recently the state took over marine archaeology completely, after a series of experiments with restrictive legislation, but it is too late; there is not much left to guard.

The same applies to Italy. There are too many skin divers, they have no respect for the archaeological services, and they are out of control.

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The problem is acerbated by what might be called the "culture gap" between the local authorities and the skin divers. The European educational system is much more rigid than ours. Archaeological service personnel tend to be library oriented classicists. Divers are oriented towards machinery, adventure, athletics and romance. Several excavations of great promise have failed because of the inability of the one group to communicate with, or respect, the other.

One famous European archaeologist, one of the founders of marine archaeology, actually forbade his assistants on an underwater excavation to dive. He thought diving was a dangerous, undignified pursuit best left to the less gentle classes.

Yugoslavia is about the only country to have solved the problem. After a series of unfortunate experiences in the 1950's, they have virtually forbidden skin diving of any kind. Diving equipment is sealed by the customs and kept watch over in foreign yachts, which have to follow a prepared itinerary. This kind of control is possible in an authoritarian state, but not possible, even if it were judged desirable, in countries where freedom of the seas is a traditional right.

Israel too is in reasonably good shape as concerns this problem, but Israel is a small country, its standards of education very high, its Mediterranean coastline only 117 miles long, and divers and archaeologists often know one another personally. Indeed they are, in some instances, the same people. Here there is a vigorous program to instruct divers and historians in the techniques of marine archaeology. Here, too, surplus artifacts are drawn, photographed, numbered, and sold through the archaeological service to individual purchasers, be they private people or museums. This is a good solution to an old problem - one good example of an amphora from the wreck of a wine carrying ship, for example, is enough for one museum. The biggest museum in the world has no use, and likely no storage facilities, for another thousand or so identical amphoras. The sale of objects, whose location is then known should they be needed for study, produces money which is then used in further museum or excavation studies.

Greece and Turkey have laws regarding marine antiquities, but their problems are similar to those in Italy. Enforcement is impossible, and as tourism increases and more local people learn to dive, the portable remains of shipwrecks are fast disappearing.

As Mr. South says, the problem in the United States (and the same in the Mediterranean) is that laws must be made which are "strong enough to protect the sites while at the same time not being so prohibitive as to be unenforceable, and therefore virtually useless."

A state law regarding marine archaeology should probably accomplish several things. It should

- 1) make it impossible for any group or person to rob any wreck and get away with it,

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- 2) reward anyone who helps the state preserve or excavate wrecks, and,
- 3) encourage supervised undersea research. This can only be done by creating a state department of Marine Archaeology, financed by the state, manned by a staff of qualified people.

It should be brought to the attention of the legislators that wrecks given to commercial salvage companies, whether or not for "archaeological" salvage, will be destroyed, for the reasons given above. No commercial salvager has yet seen that material was properly published, either in America or in Europe. There is a limit to the time available for law making; too much delay will lose the state's heritage of marine objects forever.

If the wrong kind of legislation is made, and a situation arises in which skin divers and archaeologists are set against each other, then it is equally sure that the shipwrecks will be lost, as happened in France.

Possible Solutions

If the state were to set up a department of Marine Archaeology, it might then arrange a commission of marine archaeology, to consist of representatives of divers, historians, and educators. The commission could decide on which wrecks should be protected by the state, and which could be awardable to commercial salvage companies. The commission budget should allow research into the problems of legislation, in order to recommend suitable legislation. (Such a study might be undertaken as a thesis project by a group of graduate students.)

Excavations

It is true that underwater work is expensive, but it becomes much less so when done under university auspices as a non-profit enterprise. Little capital investment for heavy equipment would be necessary. There are legal channels for loan of equipment from federal agencies to universities.

If the state furnished operating cash and found a way of making some kind of awards to companies and people who helped with contributions in "kind", expenses are reduced still further.

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Personnel

If the state can provide a trained cadre of people, it should be easy to recruit volunteers. The skin divers and would-be salvagers of wrecks, the same people who tend to destroy wrecks, are generally interested in history and glad to be trained in the techniques of under-sea archaeology. Generally they are as glad to see something in a museum for their children to study as on their mantelpiece for their children to break.

The paid personnel could be recruited from divers and historians who are most interested in marine archaeology.

There are plenty of ways to reward volunteer personnel, such as scholarships, time off for state employees to work on projects, expense money for divers, with cut rate equipment, etc. The great reward is, in any case, not cash but the pleasure (and prestige) of being involved in such an enterprise.

Sojourns Ltd., of San Rafael, California, will sell you a \$995 ten day tour in the British Virgin Islands, where the possibilities of wreck robbing are vast. Collecting of "goodies" for souvenirs is normal in commercial diving tourist trips. If divers will pay that much money for probable wreck robbing, I don't see why an organized marine excavation can't recruit divers to work in South Carolina on a legitimate project they can be proud of being involved in.

The State as Its Own Salvager

Once a whole ship or cargo has been salvaged, it is then possible to decide which finds are surplus and which should be kept. There is obviously no use in keeping ten thousand Staffordshire plates and five hundred bales of rotting rubber coated canvas sheets in a state store-room. I don't believe it would be possible to finance the salvage, say, of the Mary Bowers by the sale of plates on the antique market. However, a Staffordshire plate nicely mounted on some sort of inscribed plaque and publicly presented to, say, a contractor who has loaned equipment to the project or a diver who has given his holidays, might make a very adequate and pleasing reward.

Such a plate, sold in a state museum with a certificate of authenticity, will bring a higher price than a similar plate on the antique market. I doubt that Mr. Lee Spence will find a way to profit from old rubber ground sheets, or rotting spools of historical thread. If the Vasa Museum experience is any guide, the state could profit from the judicious sale of objects that the ordinary salvager has no time to fool with.

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As mentioned above, the state's selling surplus antiquities in order to finance part of its archaeological service has been a success in Israel, for instance. It is not practiced in Mediterranean countries with unstable governments and a tradition of corruption in public affairs, for obvious reasons. But there seems no reason why state owned antiquities should not be exploited for the state's profit, when the exploration is carefully and intelligently controlled.

Mr. Spence says, "...try to think for a minute what amount of money you could get for the intact items that you find on one of your digs". Personally, as I said above, particularly with reference to the silver coin wreck, I doubt it. The market value of such antiques can only be maintained on a high level, with the sellers of the objects in question rigidly controlling the market, as do the diamond miners of South Africa. Only the state or a very large company has the ability to do this.

I would like to close by comparing the cost estimate given by Mr. Spence in regard to his expedition with the same figures from an expedition which I recently directed. These illustrate the difference in cost between a private salvage job and an excavation supported by both the state and a university.

At Torre Sgarrata, near Taranto in southern Italy, we worked for part of 1965 and the whole summers of 1967 and 1968. The job was the clearing, drawing, photographing, excavating and storing of a big Roman ship with a cargo of marble. We had all the equipment mentioned by Mr. Spence, plus the use of a 500 ton crane barge, several tugboats, and a dry dock, together with naval and museum personnel. Over 50 divers worked for more than 2000 hours on the bottom.

We raised 160 tons of cargo, and the remains of a 300 to 400 ton ship and warehoused the finds, including building preservation tanks for the wood. The whole project cost under \$40,000 in cash. It was, furthermore, carried out 300 sea miles from the expedition's operating base, across two complicated customs frontiers, in three languages, and with the organization's formal headquarters across the Atlantic.

This was possible because the project was a cooperative one, supported by local authorities on every level, market gardener to mayor. The navy, simply by being helpful, saved us thousands, as did airlines, travel offices, hotels, campsites, etc. It is much more difficult to raise money in Italy than in the United States. Particularly in southern Italy, the people are poor.

What made the project possible was a reasonable amount of cash which acted as the grease to make the expedition run, and made it possible to use the good will of the community. Certainly this sort of local cooperation should be even easier to achieve in South Carolina. Such a project there could not reasonably cost the state of Carolina more than the Torre Sgarrata cost its sponsors. It might even cost less, because the logistic problems are so much simpler.

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Calculating the Torre Sgarrata job as a commercial enterprise at the approximate local rates for divers, cranes, boat hire, space rental, etc., the cost would have been in the neighborhood of \$180,000. Considering that many of these costs would be about doubled in the United States, one agrees that Mr. Spence's figures are not far off. And the Mary Bowers job is likely a more difficult one than at Torre Sgarrata, where the water was clear, there was no current, and the wreck had fallen to pieces, so that the largest object that had to be raised weighed under ten tons.

The foregoing discussion may seem like an attack on Mr. Spence. I do not mean this to be so. A whole new branch of archaeology, that of Mycenaean studies, was founded by Heinrich Schliemann, who also had the courage to remember his dreams, and spent most of his life becoming a millionaire so that he could afford them.

It is right to dream, and it would be the worst kind of mistake on the part of the state to discourage the big dreams of men like Mr. Spence, and to let a project requiring that sort of enterprise fall into the hands of what Mr. Spence's friend terms "some bloody historical society" which might lay the dead hand of unimaginative and stereotyped thinking on Mr. Spence's courage and ability.

Given, then, that it is not humanly possible for Mr. Spence to make a profit out of the Mary Bowers as other than ruthless and destructive salvage, what is the solution? Now that he owns the Mary Bowers, he is in a position to do what he likes with her. As an independent person, all that he can do is get the goodies and leave the junk, to the detriment of the people of South Carolina.

If it is desirable that the wreck be saved, then some sort of compromise must be reached between the state and Mr. Spence, some arrangement which can both save the wreck and reward Mr. Spence for his ingenuity, enterprise, and time.

The exact terms of such a compromise are more in the realm of the lawyer than of the archaeologist. A rough outline might be that Mr. Spence, acting in the name of the state, would take over the job as a non-profit enterprise. With the backing of the state he could campaign for funds and help for the salvage of the wreck and restoration as well, if this is technically possible. If he were guaranteed a reasonable salary, with a bonus incentive for raising the ship without destroying either ship or cargo, and her further preservation, then everyone would benefit. A provision could be made that he lose the contract for improper performance of it.

The above is a very idealistic view. No state in the union seems to have succeeded in separating salvage from archaeology, and most European countries haven't managed either. Yet the thump of dynamite echoes along the American coastline, and no wreck is safe from well equipped salvage enterprises.

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No federal agency seems as yet to have studied the rate of destruction of marine antiquities in the United States. Judging from the general feeling among people in the antique salvage "archaeology" business, one can suppose that the easy wrecks will be gone in five years. Unlike our wasted or polluted natural resources, the dynamited wrecks cannot be renewed, no matter how much good will be brought, too late, to the scene. The wrecks must be saved now, or never. Their fate is partially in the hands of the lawmakers.

RECOMMENDED READING

Bass, George

1966 Archaeology Under Water. Thames and Hudson, London.

Clarke, Arthur

1964 The Treasure of the Great Reef. Harper and Row, New York.

Frantzen, Anders

1962 The Warship Vasa. Bonniers, Stockholm.

Throckmorton, Peter

1969 National Geographic (February, 1969). Ancient Shipwreck Yields New Facts and A Strange Cargo, and (mimeographed) Torre Sgarata.

University of Pennsylvania Museum

1969 Report. Marine Archaeology Section (gives statistics of cost, etc.).

University of Pennsylvania Museum

1970 Shipwrecks and Archaeology. Marine Archaeology Section. Atlantic Little Brown, Boston.

Note that most of the material in the article is footnoted in Shipwrecks and Archaeology, 1970, and Archaeology Under Water, 1966.

UNDERWATER SALVAGE ADMINISTRATION:
THE SOUTH CAROLINA CASE

Robert L. Stephenson

The South Carolina legislature of 1968 passed "An Act to Provide for State Control of Salvage Operations in Navigable Waters within the State and in Certain Waters of the Atlantic Ocean..." In this act the South Carolina Department of Archives and History was designated as the agency to administer this law. The legislature of 1969 amended the law to substitute the Institute of Archeology and Anthropology, University of South Carolina, for the Department of Archives and History. Thus, on May 30, 1969, I became responsible for administration of a whole field of endeavor about which I knew very little. Since then I have read a great profusion of words, talked to a great many people, and tried to analyse a number of specific situations. From all of this, I have learned much and have become bold enough to establish a means for administering this law with some chance of success in dealing fairly with the interests of the state and with the interests of individuals who apply for permits and licenses. Perhaps a brief resume of what has gone into this procedure will be helpful to others and illuminating to those who intend to conduct operations in South Carolina. Hopefully it will bring forth some comments helpful to a better administration of this law.

It was immediately obvious that a set of uniform rules and regulations must be devised to systematically apply to the law. South Carolina has no antiquities law applying to the land so there was no state precedent. There is such a precedent, though, in the Uniform Rules and Regulations for the Federal Antiquities Act of 1906. These rules and regulations, then, became the basis for the new rules and regulations governing the underwater law of South Carolina. The rules and regulations of North Carolina, of Florida and of Texas were also used as a basis for more specific sections. Hopefully, the best of all of these have been combined into this new document.

In January 1970, I attended a most fruitful Seminar (or bull session) in Bethlehem, Pennsylvania, at the annual meeting of the Society for Historical Archeology. Carl Clawson, of Florida, organized this and while his original purpose may not have been achieved I found it a most productive and helpful session. Many of the people throughout the country who are facing these same problems were there and freely discussed their ideas.

Some of those ideas were most interesting and developed into a real discussion of the whole philosophy of underwater archeological salvage. There were those who felt that licenses and permits to divers who were not trained archeologists should not be granted at all. The philosophy expressed was that these are all antiquities in the public domain and that, just like an Indian burial mound, should be investigated only by fully qualified archeologists in the public interest. "You wouldn't issue

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permits for pothunters to dig an Indian mound or village and let them take a percentage of the artifacts, and the same idea applies here" it was said. This is a valid concept and worthy of very serious consideration. It was the philosophy expressed by a majority of those at the session.

On the other hand the philosophy was expressed that we must deal with reality. The realities are these. State agencies are not now and are not soon apt to be set up to explore and excavate these underwater antiquities with trained staff and proper techniques. Underwater exploration by untrained people is becoming immensely popular. People are diving and they are recovering things. Without a tremendous policing capability many submerged antiquities will be recovered or destroyed without any official notification to the state. This is not all by commercial divers but often simply by hobbyists who have no way of knowing any better. Also natural marine erosion is not helping to preserve these submerged antiquities.

A solution to this is to grant licenses and permits to competent divers who will agree to at least try to learn and practice basic rudimentary archeological techniques. These people are then given a percentage of the recovered objects as compensation for their efforts. At least, they will recover some of the submerged antiquities in at least a semi-scientific manner and will tend to aid in policing and keeping unlicensed divers from looting the sites.

This is, in effect, an attempt to educate and to develop semi-skilled, amateur archeologists from the divers who otherwise would know nothing of even the basic ideas of underwater archeology. It is comparable to developing, on land, amateur archeologists out of the mass of vandalizing pothunters. It must be understood, though, that in the rules applying to these people and stated clearly in their licensing agreements that certain basic archeological techniques would be practiced.

This is a sort of compromise philosophy between the ideal situation and the reality of our present capability for achieving this ideal. I believe it is the best solution that can be attained under the present circumstances.

With all these things in mind, I have devised a set of Uniform Rules and Regulations by which the State of South Carolina Law can be administered. It must be kept in mind that I have not one dollar of specific funds for administering this law except for the token amounts to be derived from license fees. Neither do I have any personnel that can be assigned, even part time, to administration of this law. All expenses incurred and staff time expended has been, and will continue this year to be, taken from funds essentially intended for other purposes. Specific funding for this must be arranged in next years budget and sufficiently so that at least one full-time staff position can be assigned to this work.

On May 20, 1970, a meeting of all those in South Carolina who are seriously interested in obtaining permits and licenses was held in Charleston. There were 20 people present including a representative of the Department of

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Archives and History, Mr. Barney Slawson, and a representative of the State Department of Parks, Recreation and Tourism, Mr. Janson Cox, and myself. The new Uniform Rules and Regulations and the law itself were intensively discussed and some good comments were made. Some of these comments suggested useful changes and these changes have been made. I believe that, at least, these people have a clear understanding of the law and of the Uniform Rules and Regulations governing it. In general, there was rather clear concurrence on these matters and these people seemed willing to accept them. Hopefully they will abide by them.

South Carolina now has five permits and licenses outstanding. Three licenses were issued prior to the development of the Uniform Rules and Regulations. Two permits have been issued since. One of the licenses is now pending for renewal. The holders of the two permits are complying completely with the Rules and Regulations. The previous licensees are in compliance with the requests made of them at the time of licensing.

This has been a difficult and trying time of development in this field. I believe the basic groundwork has been accomplished for a fair and reasonable solution to the administration of this law. All problems are not solved and there will continue to be much to do but I do think that at least we have made a proper start. This, then, is what one state is doing in the field of Underwater Archeological Salvage.

A copy of the South Carolina State Law and the Uniform Rules and Regulations governing it is printed below. Comments would be welcomed.

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THE LAW

(R1518, H2824)

An Act To Provide For State Control Of Salvage Operations In Navigable Waters Within The State And In Certain Coastal Waters Of The Atlantic Ocean And To Provide A Penalty.*

Be it enacted by the General Assembly of the State of South Carolina:

SECTION 1. Subject Chapter 3, Title 54 of the 1962 Code; the title to all bottoms of navigable waters within one marine league seaward from the Atlantic seashore measured from the mean low watermark, and the title to all shipwrecks, vessels and all things therein, including but not limited to cargoes, tackle, and underwater archaeological artifacts or any other thing of value which have remained unclaimed for more than ten years on such bottoms is hereby declared to be in the State of South Carolina.

SECTION 2. (a) The custodian of shipwrecks, vessels, cargoes, tackle and underwater archaeological artifacts shall be the South Carolina Institute of Archeology and Anthropology, University of South Carolina, which is empowered to promulgate such rules and regulations as may be necessary to preserve and salvage such underwater properties.

(b) The custodian of any other thing of value set forth in Section 1 shall be the Budget and Control Board of the State, which shall have the power to promulgate rules and regulations for such purpose.

SECTION 3. Any person desiring to conduct exploration, recovery or salvage operations, in the course of which any part of a derelict or its contents or other archaeological site may be removed, displaced or destroyed, shall first make application to the Institute of Archeology and Anthropology for a permit to conduct such operations. If the Institute shall find that the granting of such permit or license is in the best interest of the State, it may grant such applicant a permit or license for such a period of time and under such conditions as the Institute may deem to be in the best interest of the State. Such permit or license may include but need not be limited to the following:

- (a) Payment of monetary fee to be set by the Institute;
- (b) that a portion or all of the historic material or artifacts be delivered to custody and possession of the Institute;
- (c) that a portion or all of such relics or artifacts may be sold or retained by the licensee;
- (d) that a portion or all of such relics or artifacts may be sold or traded by the Institute;
- (e) any other reasonable conditions constituting fair compensation to the permittee and protection of property rights of the people of the State.

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SECTION 4. Any person desiring to recover anything of value other than shipwrecks, vessels, cargoes, tackle and underwater archaeological artifacts shall obtain a permit from the Budget and Control Board which shall contain such terms as such board shall deem necessary.

SECTION 5. Permits may be renewed upon such terms as agreed to by the applicant and the agency concerned. Holders of permits shall be responsible for obtaining permission, if any is required, of any Federal agencies having jurisdiction, including the United States Coast Guard, the United States Department of the Navy and the United States Army Corps of Engineers.

SECTION 6. Any funds received by the Institute of Archeology and Anthropology or the Budget and Control Board under the terms of Section 3 may be allocated for use by the Institute for continuing its duties under this act, subject to the approval of the Budget and Control Board, or the Board may direct that all or any part of such funds be paid into the general fund of the State.

SECTION 7. All State and local law enforcement agencies are hereby empowered to assist the Institute, the Board, and the permittee.

SECTION 8. Any person violating the provisions of this act shall be guilty of a misdemeanor and, upon conviction, shall be punished by a fine in the discretion of the court or by imprisonment for a term not to exceed two years.

SECTION 9. This act shall take effect upon approval by the Governor.

In the Senate House the 14th day of June.

In the Year of Our Lord One Thousand Nine Hundred and Sixty-eight.

John C. West,

President of the Senate.

Solomon Blatt,

Speaker of the House of Representatives.

Approved the 14th day of June, 1968.

Robert E. McNair,

Governor.

*As amended by Act of the South Carolina Legislature (R379, S193) Statutes at Large of South Carolina of 1969, No. 262.

Approved the 30th day of May, 1969.

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UNIFORM RULES AND REGULATIONS

PREScribed BY THE STATE ARCHEOLOGIST TO CARRY OUT THE PROVISIONS OF
THE "ACT TO PROVIDE FOR STATE CONTROL OF SALVAGE OPERATIONS IN NAVIG-
ABLE WATERS WITHIN THE STATE AND IN CERTAIN COASTAL WATERS OF THE
ATLANTIC OCEAN..." Approved June 14, 1968, as amended May 30, 1969.

1. AUTHORITY: In accordance with Section 2 (a) of this Act as amended, the Institute of Archeology and Anthropology, University of South Carolina, (hereinafter referred to as the Institute), acting in the best interests of the State, shall prescribe and set forth the following rules and regulations to carry out the provisions of this Act and shall make such changes and emendations to these rules and regulations from time to time as, in its opinion, are to the best interests of the State.

2. DEFINITIONS: (a) The term "salvage operations" as used in this Act shall be interpreted to mean the disturbance or removal of any submerged antiquities that lie within any navigable waters of the State or that lie within one (1) marine league seaward from the Atlantic shoreline of the State measured from mean low watermark. (b) The term navigable waters includes a stream in which the tide ebbs and flows, for as far as the tide ebbs and flows as well as those nontidal waters of the State which are navigable in fact. (c) The term "archeological artifacts" as used in this Act shall be interpreted to mean any and all objects made or used by man, or fragments thereof, whether or not associated with any shipwrecks or vessels and which have remained unclaimed for more than ten (10) years. (d) The term "submerged antiquities" as used herein shall be interpreted to mean shipwrecks, vessels, and all things therein, including but not limited to cargoes, tackle, ship's fittings, personal effects, and all other archeological artifacts whether associated with any vessel or not, or any other things of value made or used by man, including treasure trove and objects of prehistoric as well as historic origin, which have remained unclaimed for more than ten (10) years and which are located at or below mean high watermark.

3. PROHIBITIONS: (a) No salvage operations, as defined above, shall be conducted by any person or organization that has not first been granted an appropriate permit or license therefor from the Institute. (b) No permit or license for the disturbance or removal of any submerged antiquities which can be preserved in situ under the control of the State and remain as objects of interest, shall be granted. (c) No permit or license for the disturbance or removal of any submerged antiquities which are, in the opinion of the Institute, a part of any archeological site on land where the State may be expected to conduct archeological research, shall be granted except in relation to and as a part of that archeological research. (d) No permit or license for the disturbance or removal of any

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submerged antiquities which, in the opinion of the Institute, are of primary scientific value shall be granted. (e) No permit or license, or any part thereof, shall be assigned or sublet.

4. APPLICATIONS FOR PERMITS AND LICENSES: (a) Each application for a permit or license to conduct salvage operations shall be filed with the Institute in writing on the standard application form provided by the State. (b) Such application shall be accompanied by a definite and specific outline of the proposed work, indicating the proposed date for the beginning of the work, the length of time proposed to be devoted to the work, and a statement of the anticipated disposition of the objects recovered and anticipated to be retained by the applicant, if any. (c) It shall also be accompanied by the name and address of the applicant, the name or names of the person or persons who will be in immediate charge of the work, the names and addresses of all persons who will participate in the work and/or who are connected otherwise with the work. (d) It shall also be accompanied by a listing of the experience, training and background of responsibility of all participants in the work, an exact listing of all equipment to be used in the work or that will be available for use in the work, and a statement of the financial preparation of the applicant for the work. (e) It shall also be accompanied by a sketch map, location map, or other pictorial description of the site or area where any and all of the work is to be done, of sufficient detail and definition as to be clearly and accurately located on a standard map or chart.

5. GRANTING OF PERMITS AND LICENSES: (a) Permits and licenses for salvage operations may be granted from time to time by the Institute to persons, corporations, companies, partnerships, agencies, or institutions that are, in his opinion, of reputable character, are qualified by underwater diving experience and training, are financially able, and are adequately equipped to carry out the salvage operations proposed. (b) Such permits and licenses are granted upon proper application, as described above, and will be in writing on the appropriate standard form provided by the State. One signed copy and one other copy shall be provided the applicant and one signed copy and one other copy shall be filed with the Institute. (c) Permits and licenses shall be of four kinds: Underwater Hobby Permits, Underwater Search and Exploration Permits, Underwater Search and Exploration Licenses, and Underwater Salvage Licenses. (d) Any permit or license under which the work has been diligently prosecuted and all rules and regulations faithfully adhered to, may be renewed for proper cause when in the best interests of the State, upon application and payment of additional fee, at the discretion of the Institute.

6. UNDERWATER HOBBY PERMITS: A non-exclusive permit may be granted to an applicant for a temporary, intermittent search or salvage operation, requiring minimal equipment, training, and experience. It is granted for a period of up to one year. A standard fee of ten dollars (\$10.00) shall be charged for any one individual applying for such a permit or twenty-five dollars (\$25.00) for any club or other organized group of individuals. The specific areas of intended activity of the applicant will be stated in

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the permit but may be so broadly stated as to include whole counties and the offshore waters adjacent to such counties. Holders of such permits are specifically forbidden to exercise the privileges of this permit in waters for which any type of exclusive permit or license has been granted and is in effect or in waters for which such exclusive permit or license becomes effective during the life of that exclusive permit or license.

It is the intent of these Rules and Regulations to limit the recovery of submerged antiquities under this permit to small scale "hobby" activities. Commercial salvagers, firms, partnerships, corporations or individuals seeking to recover and sell submerged antiquities, or to otherwise make monetary profit from them, shall not be granted this type of permit.

A written report of activities including a complete listing of all recovered objects and a diagram or other description of the place or places from which they were recovered shall be made to the Institute at the end of each calendar month during the life of the permit. All objects recovered under the authority of this permit during any calendar month will be made available to the Institute for inspection during that month or no later than the end of that month. The Institute will grant to the permittee all, a portion, or none of the said objects either immediately, or after a reasonable period of time for study, research, and photographing of said objects.

7. UNDERWATER SEARCH AND EXPLORATION PERMITS: An exclusive permit may be granted to an applicant for the purpose of exploration of an area of less than one (1) nautical mile square in order to locate and evaluate submerged antiquities for later salvage. It is the intent of these Rules and Regulations to limit this type of permit to an intermittent operation requiring minimal equipment but extensive experience and training. The disturbance or removal of submerged antiquities under this permit is expected to be minimal and of a nature only to allow for evaluation and interpretation of the antiquities found in the specified area. Such permit shall be granted for a period of not to exceed ninety (90) days and a standard fee of twenty-five dollars (\$25.00) shall be charged. A written report of activities including a listing of all objects recovered and a diagram, chart, or other description of all submerged antiquities located shall be made to the Institute within three (3) days of the completion of each diving operation made under the privileges of this permit, or if the operations are more-or-less continuous at the end of each calendar week of such operation. All objects recovered under the authority of this permit shall be made available to the Institute for inspection within ten (10) days of their recovery. The Institute will grant to the permittee all, a portion, or none of the said objects either immediately or after a reasonable period of time for study, research, and photography of said objects.

8. UNDERWATER SEARCH AND EXPLORATION LICENSE: An exclusive license may be granted to an applicant for the same purposes and under the same

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conditions as an Underwater Search and Exploration Permit with the following exceptions: (a) The area for which such license is granted is larger than one (1) nautical mile square but in no case greater than nine (9) nautical square miles. (b) It may be granted for a period of up to one (1) calendar year. (c) A standard fee of one hundred dollars (\$100.00) shall be charged.

9. UNDERWATER SALVAGE LICENSE: An exclusive license may be granted to an applicant for the purpose of conducting a well-planned, large-scale, continuing, underwater salvage operation with experienced personnel, complex equipment, and major financial support. It is granted for a period of not to exceed one (1) calendar year. A fee ranging from one hundred dollars (\$100.00) to five hundred dollars (\$500.00) shall be charged, the rate within this range to be determined by the Institute at the time of granting of the license and based upon the proposed size of the operation and other pertinent factors. A written report of all activities of the licensee pertaining to the exercise of the privileges granted under this license shall be made to the Institute on the last calendar day of each month during the time the license is in force. This report shall include a listing of every diving operation by day and hour, a listing of all objects recovered together with notations concerning the precise locations from which the objects were recovered and a sketch, plan drawing, or other pictorial representation of the appearance of the submerged antiquities in that location as of the beginning of the reporting period. It will also include a listing of all changes of personnel connected with the salvage operation, changes of, or additions to, equipment, and/or financial support for the operation, and any other pertinent information. This report will be due in the office of the Institute no later than the tenth (10th) of the month following the month reported.

All objects, recovered under the authority of this license shall be made available to the Institute for inspection within thirty (30) days of recovery and shall remain available to it for a period of sixty (60) days thereafter for study, research, and photography. The Institute will grant to the licensee all, a portion, or none of the said objects recovered, either immediately or after a reasonable period of time for study, research, and photography of said objects.

10. OWNERSHIP OF RECOVERED OBJECTS: In accordance with the provisions of the Act it is clearly understood that all submerged antiquities as defined herein or objects that have been, in the immediate past, submerged antiquities are the property of the State of South Carolina. Such submerged antiquities as are from time to time recovered under proper permit or license may be granted, in part, to the permittee or licensee as proper compensation for his efforts in recovering such objects and the title to and ownership of these objects then is transferred to that permittee or licensee. Determination of which of the recovered objects will be granted to the permittee or licensee will be made by the Institute acting in the best interests of the State and giving due consideration to

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the fair treatment of the permittee or licensee.

11. RESTRICTIONS ON PERMITS AND LICENSES: (a) No permit or license shall be granted any applicant for a larger area than he can reasonably be expected to fully and adequately investigate and exploit within the time limits and other terms of his permit or license. (b) No applicant will be granted more than three (3) permits or licenses, or combinations thereof, which will be in effect at the same time, and it is the intent of these rules and regulations to limit concurrent permits or licenses, or combinations thereof, to one permit or license to a single applicant insofar as this is practicable and in the best interests of the State. (c) No objects recovered under the authority of any type of permit or license shall be disposed of by gift, sale, discard, or in any other way until the Institute has inspected them and released them for that purpose. (d) Failure to begin work under the terms of a permit or license within the first third of the life of that permit or license, or failure to diligently prosecute such work after it has been started, or to faithfully comply with any of the provisions of the application, or the permit or license, or of these rules and regulations shall be reason for revocation of the permit or license and any permit or license may be revoked, for cause, at the discretion of the Institute. (e) Any object or objects that have been or are submerged antiquities, as defined herein, that have been recovered, taken, collected, or otherwise disturbed, contrary to the terms of a permit or license, or without a permit or license, may be confiscated wherever found and at any time by any South Carolina Law Enforcement Officer or by any member of the staff of the Institute, and may be disposed of as the Institute shall determine by deposit in the proper state repository.

12. ENFORCEMENT OF THE ACT: (a) Any State of South Carolina Law Enforcement Officer or any duly authorized member of the staff of the Institute or of the staff of the South Carolina Department of Archives and History, or of the South Carolina Department of Parks, Recreation and Tourism may, at any time, examine the permit or license of any person or organization claiming privileges under this Act and these rules and regulations, and may fully examine all work done under such permit or license and may apprehend or cause to be arrested any person or persons suspected of being in violation of any part of this Act or these rules and regulations, and may confiscate any antiquities in the possession of such person or persons that have been recovered as a result of such suspected violation. (b) The Institute may seek consultation and advice on any aspect of the granting or supervision of a permit or license or on any other aspect of the carrying out of the provisions of this Act and may act upon that advice or consultation in the best interests of the State. He may request additional information, documentation, or references, from any applicant for a permit or license or from any permittee or licensee at any time.

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13. PRESERVATION OF ANTIQUITIES: Any applicant for a permit or license shall clearly demonstrate his capability for proper preservation and conservation of any antiquities that he may reasonably be expected to recover under the terms of his permit or license and shall properly and adequately take such preservation and conservation measures as are appropriate for all such antiquities as are recovered under the terms of his permit or license when granted.

14. COMPLIANCE WITH SAFETY AND OTHER REGULATIONS: (a) Any permittee or licensee shall fully comply with all normal safety rules and regulations ordinarily applying to any underwater diving or other underwater activity as exercised under the privileges of his permit or license and the State does not hold itself in any way liable or responsible for any accident or injury sustained by any person or persons or loss or damage to any equipment in any way connected with such license or permit. (b) Each licensee or permittee shall be expected to comply fully with any and all federal regulations pertaining in any way to submerged antiquities or underwater diving operations. (c) Each permittee or licensee shall be expected to properly mark and to protect against encroachment by others, any and all sites and areas for which his permit or license is granted and the State in no way holds itself or may be held responsible for such marking or protection except as may be incidental to the administration of the Act and of these Rules and Regulations.

15. ARCHEOLOGICAL DATA REQUIRED: (a) Since it is solely the purpose of this Act to preserve the historic and scientific values inherent in submerged antiquities for the benefit of all of the people of the State, it is only incidental to this purpose and as a means of achieving this purpose that permits and licenses as described above may be granted and that portions of the recovered objects may be granted in ownership to the permittees or licensees. (b) As a means of preserving the historic and scientific values of the submerged antiquities each and every permittee or licensee is required to exercise all data recording techniques of which he is capable within the framework of his operation and is expressly forbidden to disturb or remove submerged antiquities without making as much of a record of their in situ positions and locations as he is able. (c) Minimal records that are required consist of (1) plan or sketch drawings of the objects in situ before disturbance or recovery. (2) Photographs of same whenever visibility permits. (3) Sketches or plan drawings of large submerged antiquities, such as shipwrecks, in detail showing positions of decks, bottoms, holds, super-structures, stern, bow, cargo and other features and their relationships to each other. (4) Photographs of same whenever visibility permits. (5) Narrative descriptions of these features. (6) Narrative or pictorial descriptions of the surroundings including any adjacent objects, depth of water, nature of bottoms such as quantity of mud and depth the objects are sunk into the mud. (7) Photographs of all recovered objects immediately after recovery. (8) Records of what part of the shipwreck or area of salvage each recovered object came

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from i.e. from the hold, from the deck, from a cabin, from near the shore, etc. (d) A permittee or licensee will not be permitted to recover underwater antiquities selectively, on any basis, but is required, insofar as is feasible, to recover all antiquities within the purview of the permit or license. He shall not select only salable objects to recover nor only one kind of object but shall recover all objects located including broken objects, fragments of objects, prehistoric objects, and other antiquities in his search area. If some objects, such as a ship's hull, are too large to recover he is required to describe those objects that were not recovered. (e) A permittee or licensee is expressly prohibited from using underwater explosives or other grossly destructive devices, which devices shall be determined by the Institute, in any aspect of the salvage or exploratory work and is expressly prohibited from causing damage or destruction to underwater antiquities that in any way prevent or limit the potential recovery of those antiquities.

16. PERMITS AND LICENSES FOR AREAS AND SITES: Hobby permits and Search and Exploration permits and licenses shall be granted for specifically described areas. Salvage licenses shall be for specifically described localities, sites or object concentrations, located by definite coordinates, rather than for areas.

17. SUPERVISION BY THE STATE: (a) To afford adequate protection for the interests of the State, it shall be the policy of the Institute to limit the number of permits and licenses to be granted, to those that can be properly supervised and administered by the duly authorized agents of the State. (b) The Institute by its designated agent may at any or all times, visit and be present at any or all salvage operations including diving operations, warehouse facilities or any other phase of the operation for which a permit or license has been granted. (c) The representative of the State will visit such operations only when a representative of the permittee or licensee is present. (d) All permittees or licensees shall be required to comply with all reasonable requests of directives addressed to them by the Institute or its designated agent with respect to the operations authorized by said permits or licenses. (e) At all times there shall be one person designated by and acting for the permittee or licensee aboard any vessel or present at any phase of the operation going on under the permit or license who shall be responsible for the work and who shall be familiar with the rules, regulations, and directives concerning the work and who shall be responsible for compliance with such rules, regulations and directives to insure the preservation of archeological and historical data.

18. BOATS TO CARRY IDENTIFICATION: (a) All boats are required to carry identification as required by the State while operating under permit or license. (b) In his application each permittee or licensee shall furnish a list of all vessels to be used under the permit or license, including full description and registration number of the vessel. No vessel shall be used under any permit or license that is not so listed. (c) Each vessel used under any permit or license shall be marked by a flag to be furnished by the State and by a number designated on the permit or license. The flag shall

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be a minimum of two (2) feet wide by three (3) feet long, consisting of a florescent material, and shall be displayed on the vessel so that it is clearly visible in all directions. The identification number shall be in block style with a minimum height of two (2) feet and of sufficient width to be clearly read at a distance. The color of the identification number shall contrast with the background on which it is displayed. The number shall be placed on a horizontal surface so as to be prominently visible and clearly readable from the air. Each vessel operating under a permit or license shall display the flag and identification number continuously from the time of leaving port to the time of return to port. (d) Written identification provided by the State shall be carried at all times by each vessel operating under a permit or license.

19. EMPLOYMENT CONTRACTS AND AGREEMENTS: (a) It is the intention of the State to limit permits and licenses under this Act, insofar as is practicable, to those persons, corporations, agencies, companies, partnerships, or institutions who will themselves conduct the work and who will remain solely answerable to the State for the conduct of the operations and the proper accounting of materials located or recovered under the terms of the permit or license. (b) An employment contract, as defined for the purposes of these rules and regulations, is a contract or agreement between the principal party of the permit or license and others who purport to be supplied and equipped to perform the actual exploration and/or salvage for the permittee or licensee. The State will not approve such employment contracts and will consider any permit or license subject to cancellation if the permittee or licensee does not retain full responsibility for the actual operations conducted under the permit or license.